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SOME THEORETICAL AND PRACTICAL ASPECTS OF SAMPLING PROCEDURES IN RESEARCH

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What then is a good experiment? It is that which informs us of something besides an isolated fact; it is that which enables us to forsee, that is, which enables us to generalize.

Poincaré.

INTRODUCTION

With a sample of well over 1,000,000 cases, with a standard technique for collecting, for recording, and for tabulating the returns, the prediction was made that the 1936 presidential election would see Alfred Landon an easy victor over Franklin D. Roosevelt.

The 1936 fiasco of the *Literary Digest* is very well known and is recalled here to make one very important point: Its conclusions were probably right and its predictions highly appropriate *for the sample used.* Unfortunately, the sample collected was not representative of the total population about which predictions were to be made. The mistake lay in the selection of a sample from lists compiled in large part from telephone directories. In 1936, the possession of a telephone was highly correlated with income, and income also correlated nicely with a preference for Landon. Thus the damage was done.

The essence of this introduction is that one can uncover facts as easily, as accurately, and as reliably with a bad sample as with a good one. But when one attempts to do something with these facts, to interrelate them with others, to categorize and classify them and finally to make generalizations from them, the validity of these endeavors will vary directly with a sample's representativeness for the population under investigation.

In Poincare's famous essay, he further pointed out that

> Science is built up with facts, as a house is with stones. But a collection of facts is no more a science than a heap of stones is a house (13, p. 32).

Given, then, that facts *per se* are not science, it is imperative that the investigator ensure a legitimate foundation for his manipulations of these facts. An adequate and representative sample is essential to this foundation and it is with the theories and methods of such a procedure that this paper is concerned.

SOME DEFINITIONS

In sampling terminology, a *sample* represents a selected part or subset from a total larger *population* or *universe.* The particular population from which a sample is drawn is often referred to as the *parent* population. The measurable aspects of a population are known as its *parameters* (*e.g.*, the mean, the number of cases, the variability, etc.) and the measures derived from the sample of this population are *estimates* of these parameters. Thus, one may select an appropriate sample from a parent population and calculate the mean within this sample. If the sampling has been done properly, the

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sample mean is a good estimate of the population mean. Just how good it is can be stated in terms of probability statistics (cf. 5, pp. 73–75, 7, pp. 180–190).

There are many techniques for selecting a sample. Exactly what technique is chosen will depend upon two theoretical issues: a) the purpose of the research and b) the nature of the parent population (not to mention, of course, the practical matters of time and money). Before discussing these theoretical factors, we may enumerate briefly the major types of sampling techniques.

TECHNIQUES OF SAMPLING

Although there are many techniques available for the selection of a sample, one characteristic that should be present in each is some kind of randomization. The principle of complete random choice is satisfied only if the experimenter can demonstrate that every member of the parent population under investigation had an equal chance of being chosen for the sample.

The logic behind a random sample may be illustrated by describing the construction and use of a table of random numbers. A table of random numbers consists of numbers which have been chosen from a larger pool of numbers; the chance of any number being chosen from this pool was precisely equal to the chance of any other being chosen. Thus, the probability of any number being chosen is equal and, finally, the probability of encountering any number when using the table is also equal.

Suppose that the total patient population at a residential hospital represents the parent population which is to be sampled. To select a random sample from this group, the investigator first numbers *all* members of the parent population; these numbers are for identification purposes only. These numbers might be obtained by consecutively numbering an alphabetized list; they could as well be obtained by numbering all patients in one ward—or one building—moving on to the next and numbering there, and so on. The only requirement is that each member of the parent population have an identification number different from that of every other member. At this point, a table of random numbers is entered. If the desired sample is to have, for example, 100 patients, the investigator would successively draw from the table of random numbers the first 100 different numbers he encountered. Thus, number 57 from the table would select patient number 57 from the parent population as one member of the sample. Since the probability of encountering *any* number in a random number table is precisely equal to the probability of encountering any other, the selection of 100 numbers is completely random and, hence, the selection of 100 sample members equally so.

There are many other techniques, in addition to the use of tables of random numbers, for the selection of a random sample. None are as inherently valid as the use of random number selection since they will require the satisfaction of certain criteria which the random number method does not; there are times, however, when tables of random numbers cannot easily be applied and when such other techniques are highly useful. The reader desirous of further information on such methods might consult such sources as Kendall and Smith (8), or the excellent chapter by Kish (9).

Randomization is essential in sampling procedures for two reasons. The first, concerned with statistical reasoning, hinges on the fact that it is impossible to gain an accurate index of the precision of an experiment unless the sample selection procedure was random; without such an index it is impossible to make accurate quantitative judgments as to how significant or precise the results may be.[2] The second reason for randomizing is that it removes the selection of any individual sample member from the control of the investigator. Thus, once the parent population has been specified, the selection of those to be included in the sample is reduced to a mechanical affair in

[2] This is a statistical problem which will not be fully discussed here. In general, the statistical estimation of precision is equivalent to the statistical estimate of the amount of random error present in an experimental condition. The mathematical derivation of formulae for this estimate is founded upon the assumption of random sampling. In brief, without random sampling or randomization, it is impossible to apply tests of significance in a precise fashion. (Cf., for example, 6, p. 26.)

which it is impossible for bias to operate. Fundamentally, there is little concern here with conscious bias; the major problem lies in the personality dynamics of the investigator. Since few experiments are carried out where the investigator has no interest in the direction of the results, it may be fairly said that few experiments are begun in which the investigator does not have a vested interest. Certainly it is unnecessary to point out that unconscious needs are always present and to emphasize that randomization guards against the operation of such needs in the process of sample selection.

While randomization is an essential in any sampling technique, the completely *random sample, i.e.,* a technique where sampling involves *only* random choice, is a technique in and of itself. It is probably the most straightforward technique available since with only minimal specifications of the population being sampled, *viz.*, patients at *this* hospital, students at *that* university, the sampling procedure described above may be used. Despite its theoretical simplicity, however, the method does pose certain problems. On the one hand, a completely random sample is equivalent to a guarantee that every member of the sampled population had an equal chance of being chosen; to support such a criterion would require an extensive coverage of potential subjects and, often, inordinate amounts of both time and money. In addition, with information of relevant (or even potentially relevant) population characteristics, a random sample may be unnecessarily inefficient.

Some form of *stratified sampling* is usually undertaken when a sample is used to represent a population about which one has information concerning various sub-classes or strata. Here, unlike the completely random sample, the investigator does not specify only a single parent population but rather a series of sub-populations or *strata* which make up the parent population. Random sampling is then instituted within the strata—each stratum being sampled independently of each other—rather than within the total parent population.

Suppose that a political poll is undertaken in a city to predict the outcome of the election for mayor. Suppose that the city requires permanent personal registration in order to be eligible for voting privileges and that the registration asks each registrant to indicate his political preference. A tabulation of all registration forms reveals 50,000 voters: 26,000 Democrats and 24,000 Republicans; thus, the voting population is made up of 52 per cent Democrats and 48 per cent Republicans. Finally, suppose that the sample size has been set at 1,000 and the data for the poll will be collected by interviewing voters whose names have been chosen from the list of 50,000 registration forms.

How should these names be chosen? A completely random sample might be taken by numbering all the voting slips and then selecting the first 1,000 slips corresponding to the first 1,000 different numbers taken from a table of random numbers. In this case, the parent population is specified only as the total number of registration slips and no strata or subclasses are separately indicated. It is in such a case that the weakness of the random sample becomes most apparent since such a sample may distort what we actually wish to measure. A random selection of cases will not guarantee that the obtained sample will contain 52 per cent Democrats; it is probable that it will contain just about 52 per cent but we are never sure that this will be the case. In point of fact, there is almost one chance in three that the percentage of Democrats in the sample will range anywhere from 50 to 54 per cent; hence, it is possible—although not probable—to conduct the poll on a sample containing equal numbers of Republicans and Democrats although the population for which we wish to predict will give the Democrats four per cent more weight in the actual election. Although even less probable, there is still one chance in five that the percentage of Democrats in the obtained sample will go either as low as 49 or as high as 55 per cent. It is clear that if the former event does occur, we conduct the poll on a sample which reverses the actual population parameter we wished to sample.

Unlike a completely random sample, how-

ever, the stratified sample *will* guarantee that we obtain a sample with precisely the correct percentages represented. In this case, the parent population of 50,000 registration slips would first be divided into two strata: Democrats and Republicans. Knowing that the population parameters for these strata are 52 and 48 per cent respectively, we also know that a sample of 1,000 should contain 520 Democrats and 480 Republicans. A random sampling procedure is now conducted within each of the strata independently of each other; for the Democratic subsample, 520 numbers are drawn from a table of random numbers and these slips then identify the people who are to be interviewed. In like fashion 480 Republicans are identified. When the final poll is conducted, then, the Democrats will exert as much more weight in the final conclusion there as they will when the actual election takes place.

It is thus apparent that the stratified sample *always* accomplishes several things which the random sample may or may not do. In the first place, it allows us a *representative sample, i.e.* a sample in which the various strata exert the same weight as they do in the parent population. The very asset of specifying a stratum, in addition, points up the second advantage of a stratified sample in that it thus guarantees the inclusion of relevant subpopulations while—as before—the random sample only makes their inclusion a matter of probability. If, for example, an investigator wished to study the schizophrenic patient population at a given state hospital, he might want to be sure that simple schizophrenics found their way into the sample as well as the other three diagnostic categories. If one assumes that the diagnosis of simple schizophrenia is one not often used, it then becomes possible, albeit improbable, that the completely random sample might fail to include patients in this category. A stratified sample makes it impossible not to include simple schizophrenics as long as they are, in fact, represented in the hospital population. Still another asset of the stratified sample is that it allows complete control over the number of subjects to be sampled within each category. As will be discussed shortly, there are occasions when a representative sample is not desirable but where one would prefer to have a sample which has equal numbers of subjects from each of the various strata sampled regardless of the proportions of the parent population for which these strata account. Since the completely random sample selects subjects randomly from the total parent population, rather than specified strata within that population, it will typically result in a sample in which the number of subjects in a given stratum is proportional to the size or weight of that stratum in the parent population; in short, the number of cases in the sample strata will almost never be equal from one stratum to another. It is obvious that a stratified sampling procedure will make all strata equal to each other if it is desirable to have this condition.

For these, and for several other reasons which are unimportant here, a form of stratified sampling is almost always chosen in preference to the completely random sample. As noted before, however, randomization—rather than the completely random sample *per se*—should always be an attribute of whatever form of sampling is used.

Given, then, that random and stratified sampling are the two major techniques it may then be fairly stated that all other techniques of sampling are essentially variations on these two basic themes. With the possibilities of sampling randomly, of stratifying, of using different sampling methods within different strata, and with the possibility of introducing these and other facets of sampling at any of several different points within the total sampling procedure, it may be seen that there are a multitude of ways in which to establish a sample. The many available techniques are beyond the scope of this paper; the reader interested in further information may consult such sources as Blankenship (1), Cochran and Cox (3), Dixon and Massey (5), Kish (9), and Yates (15).

Before it is possible to select any sampling technique, however, the investigator must first specify the purpose of his research. For purposes of exposition, the goal of any research may be easily dichotomized into one of two different categories each of which, among other differences, requires a different approach to sampling.

ESTIMATION-RESEARCH VS. HYPOTHESIS-TESTING

The study of samples is done either a) to make estimates of various population parameters (*e.g.*, average blood-pressure, average incidence of mental illness, average variability of white-counts in normals, etc.), or b) to test for the possibility of differences between certain selected sub-groups from the larger population. While both methods utilize statistical inference, it is important to distinguish between them since they require different sampling procedures. The first type of investigation (illustrated in the earlier example of the political poll) is typically known as estimation-research while the second is simply hypothesis-testing. It is obvious that the latter procedure can be used as a method for *discovering* differences as well as a test of their prediction.

The differences in sampling procedures for these two kinds of research are a function of the use to which the results will be put. Although the differences between actual and hypothetical populations will be discussed more fully later, it may be noted that estimation-research almost always deals with actual, *i.e.*, real and existing, populations. Because of this goal, an appropriate sampling procedure will be designed to represent this real population as closely as possible. To this end, techniques such as stratified sampling are used. This approach will inevitably result in unequal numbers of subjects in the various categories being sampled. For an estimation procedure, this is expected and desirable since it is reproducing the population characteristics. In hypothesis-testing, however, an opposite approach is to be preferred. Although not a *sine qua non*, equal numbers in all groups—particularly as the number of groups goes beyond two—is always useful and convenient.[3]

An example may make the distinction between estimation-research and hypothesis-testing clearer. Suppose an investigator is interested in determining the average intelligence of the entire schizophrenic population at a given state hospital. He is not concerned with possible differences in intelligence between, say, paranoid as compared to hebephrenic schizophrenics or between acute *vs.* chronic cases. All he wants is an estimate of the intelligence of the total schizophrenic group. Despite his disinterest in the various sub-groups, however, it is important that he institute controls for such groupings; thus, if there *are* differences in intelligence from sub-group to sub-group he will want each sub-group represented in the sample to the extent that it is represented in the population. If, for example, paranoid schizophrenics are more intelligent than any other type and if a sample is selected in a way which allows a greater proportion of paranoid patients in the sample than in the hospital population, the estimate for the total schizophrenic group will be too high. Suppose the investigator decides that diagnostic sub-category and reaction-type may be related to intelligence; on this basis, he uses stratified sampling. Since there are four diagnostic sub-categories and two reaction-types, his strata yield eight sub-categories, *e.g.*, acute paranoids, simple paranoids, etc. He now sub-divides the total schizophrenic patient population at the hospital into eight lists, one for each sub-category. The size of each list tells him the proportion of

[3] Equal numbers, while always convenient, are most important in the statistical technique of analysis of variance. This technique, one of the most powerful and economical available to the researcher, rests upon certain assumptions for its use. One of these is proportionality of frequencies from group to group in a multifactor design. Equal numbers in all groups guarantee such proportionality; unequal frequencies do not, of course, always result in disproportions. If they do, however, corrections for this effect must be made; at best, these corrections are very cumbersome. A very complete discussion of the effects of and corrections for disproportionality in analysis of variance may be found in Wert *et al.* (14, pp. 211–222).

all schizophrenics belonging to a given sub-category. Knowing the desired size of the sample, he now selects patients from each list so that each sub-category contains enough patients to make it the same proportion of the sample as it is of the population. In making such selections, random sampling procedures are used. Analogous to the techniques presented earlier, random samples are drawn from each of the eight lists; each sample is drawn independently of every other.

The above sampling procedure will, of course, result in uneven numbers of patients in the various sub-categories but for the estimation-research problem with which the investigator is concerned, this will provide the best estimate of the population parameter. Now, by taking a simple mean of all the sample members, each sub-category will be contributing a weighted component toward the overall sample mean that is equal to the proportion of the overall mean which it accounts for in the total population.

Suppose, on the other hand, that the investigator's interest is not in making an estimate of intelligence for schizophrenics in general but rather in testing the hypothesis that intelligence is related to the variables of diagnostic category and reaction-type. Are schizophrenics in one diagnostic category more intelligent than those in another? Are acute cases more intelligent than chronic? In order to answer questions of this nature, the investigator will be on safer ground if, while drawing eight random samples from the eight lists, he draws the samples without respect to the population proportions such that the number of cases in each of the sample sub-categories is equal to the number in all others.

Again, it should be noted that equal numbers are not an absolute necessity in hypothesis-testing. They are always desirable for this type of research, however, and should the number of cases vary so much from sub-category to sub-category or group to group that they become frankly disproportional to each other, statistical difficulties will arise to make the analysis of the data a very cumbersome problem (cf. fn. 3).

In general, estimation-research sampling will tend to utilize the *proportionate stratified sample* while hypothesis-testing will but seldom make use of proportional representations. It is clear however, that a sample may be stratified while paying no attention to proportional loadings.

In the testing of hypotheses, adequate stratification within samples is crucial both for the design and analysis of experiments. The need for such stratification arises directly from the problem of population heterogeneity which is one of the several characteristics describing parent populations to which we may now turn our attention.

POPULATION CHARACTERISTICS

A population may be described on three counts. One may legitimately ask whether it is 1) real or hypothetical, 2) infinite or finite, and 3) homogeneous or heterogeneous. Of these three criteria, the last is of the greatest importance in this discussion. The first two, then, will be but briefly mentioned in order to cover the third more thoroughly.

When the purpose of research is the estimation of population parameters, it typically deals with actual rather than hypothetical populations. Thus, there are real populations of paranoid schizophrenic patients, of asthmatics, of malignant neoplasms. No less actual, however, are the unlimited varieties not made up of living things but comprised of anything having a common observable characteristic. Here, one can find actual populations composed of responses to the Rorschach, reactions to interpretations made during psychotherapy, statements made by Freud about the death-instinct, etc. Despite their rarity in estimation-research, however, hypothetical populations abound when the focus of investigation turns to hypothesis-testing. Whenever anything new or strange is tried upon a sample of subjects, the population of which these subjects are a sample is hypothetical, (*e.g.*, schizophrenics who have been given a new drug), since the particular "treatment" has never been utilized before. Should the new procedure be a success in the sample and move into wide-spread use, an actual population would then develop.

While many of the populations sampled are actually finite, they are—for all practical purposes—infinite. Thus, while one can conceive of a finite population specified only as adult males, it is inconceivable that all of them could ever be observed. The statistical changes made in assessing sample results of a finite population are used so rarely as to be unimportant in this discussion.[4]

Of fundamental importance in the description of a population is the decision as to its degree of homogeneity or heterogeneity. The amount of heterogeneity is not a fixed characteristic of a population but rather something that varies according to ones interest in the population.

A population is heterogeneous to the extent that it can be divided into *relevant* sub-classes or strata. A stratum is relevant only when it is pertinent or potentially pertinent to that variable for which the population is being investigated. In a previous example, the investigator specified the diagnostic category and reaction-type as being potentially relevant to an estimate of intelligence for schizophrenics in general. It is clear that he is ignoring a multitude of other strata, *e.g.*, sex, age, height, age at hospitalization, results of blood-chemistry, etc. These strata are ignored because they are held to be irrelevant to the variable under investigation. Suppose, however, that the investigator was interested in an estimate of endocrine balance for schizophrenics in general. With such an interest, the heterogeneity of the population would increase. Thus, in addition to the already defined strata of diagnostic-category and reaction-type, the investigator would probably now add strata specifications for sex and for age since these criteria seem to be potentially relevant. The nature of the majority of research interests in the behavioral sciences is such that one may safely assume heterogeneity of parent populations to be the rule and homogeneity, the exception.

Given such a state of affairs, it is the task of the investigator to make decisions as to how heterogeneous he wants his total sample to be. One may progressively diminish heterogeneity by increasing stratification or the number of sub-samples; thus, four subcategories of schizophrenia may be described within each of which patients will be more homogeneous with reference to each other than they would be were the only criterion for selection to have been that they were schizophrenic. These four sub-samples may be further increased in homogeneity by splitting them into males *vs.* females; thus yielding eight sub-classifications. These eight sub-samples might be made still more homogeneous by specifying acute *vs.* chronic reactions, thus yielding sixteen sub-samples. This continual specification can be continued indefinitely. Where one stops—or if one even begins—is dependent upon several factors; the most important, of course, is the degree of heterogeneity inherent in the population and this decision must be made in light of the area of investigation. Regardless of how many strata are specified, however, it is obvious that increments in sub-sample homogeneity are going to be accompanied by increments in both the complexity of sampling and difficulties in statistical analyses. Now if an increased homogeneity can only be bought at the cost of increased complexities, what is the rationale which makes it the unquestioned method of choice?

The answer is that increased homogeneity makes for increased precision in two ways: it decreases variability and increases specificity of results. Thus, a group of cases selected by strata specification will exhibit less variability with reference to each other than they would were they selected completely at random. A group of 35 year old,

[4] If the actual number of cases in a finite population is known and if the sample from this population is large enough to be within five to ten per cent of this total population number, various adjustments are made in the parameter estimate. (Cf. 5, 6, 7, and 15.) In general, however, such an event is improbable enough to be ignored.

male, paranoid schizophrenic patients with acute reactions will probably show less variability in, say, reactions to mecholyl than will a randomly sampled group of schizophrenics containing paranoid, simple, catatonic and hebephrenic cases, including both sexes, both reaction-types and many different ages. The stratified group will, as it were, exhibit a more homogeneous background against which any changes can be evaluated. It is obvious that changes taking place against such a homogeneous background will be more easily seen than if they took place against one that was highly heterogeneous. They can be more easily evaluated and, in fact, statistical reasoning takes this into account. Thus, a change on any given index will always be adjudged as more statistically significant when coming from a homogeneous group than an equal change coming from a heterogeneous group. In such a fashion, homogeneity increases the precision of results by decreasing the variability against which they may be exhibited. The second way in which homogeneity increases precision is but the other side of the same coin. If male schizophrenics *are* different in their reactions to mecholyl, as compared to females, the only way in which this fact is going to be apparent is by having them separated into two different groups.

An example may help to illustrate this very important point. Suppose that an investigator is interested in factors that relate to length of hospitalization for schizophrenia. Suppose that he decides to specify strata for diagnostic sub-category and for reaction-type as controls for population heterogeneity; these two strata will establish eight sub-samples. It is clear, of course, that the population may be stratified in ways other than these and it is possible that these other strata may also be relevant to the variable under investigation. For example, the population might be split into various strata on the basis of age, sex, rural *vs.* urban background, family structure, personality of parent, education, socio-economic status, adequacy of interpersonal relationships, marital status, measures on scales of ego-strength, etc. Again, they might be stratified on the basis of somatotype, reactions to adrenalin, reactions to mecholyl, varied indices of endocrine balance, differences between response to acute vs. chronic stress situations, etc. There are a host of strata such as these and among them may be some that are highly relevant to how long a period of hospitalization is necessary for sufficient recovery to afford discharge.

For the sake of this example, let us suppose that one of these variables is very important in determining how long a period of hospitalization is required. Let us say that marital status is such a factor and that married patients get better faster than those who are single; further, let us speculate that marital status has nothing to do with the kind of schizophrenia which patients develop nor with the type of reaction that ensues. Since marital status is independent of diagnostic sub-category and reaction-type, it will be randomly distributed within a sample that is selected on only these two strata. Thus, the married patient is as liable to be found in the acute paranoid group as in the chronic catatonic group; in addition, of course, the unmarried patient will be about equally represented in all of the eight groups. Suppose, now, that there is a real—but relatively slight—tendency for acute patients to get out of the hospital earlier than chronic cases (for reasons other than marital status). Lastly, assume that the investigator is ignorant of all of this.

As noted, the specification of two strata will yield eight sub-samples. How might the investigator use these samples? He might compare all the acute cases with all the chronic ones to see if there is a trend—independent of diagnostic sub-category—for one reaction-type to get better more quickly than the other. Conversely, he might combine the reaction-types and look at diagnostic sub-category so that each kind of schizophrenic would be compared with each of the others—independently, now, of the reaction-type—to see if a trend could be uncovered here. Let us concentrate on the first comparison, acute *vs.* chronic. Note that the non-specified variable of marital status is randomly distributed here so that married patients will be found about as often in the acute cases as in the chronic.

The acute cases, which have a slight tendency to get better faster, *will* do so but to a relatively subtle degree. Thus, a slight difference will exist

between the acute and chronic groups favoring the acute group. Some of the acute group, however, are married and so in addition to their slightly faster recovery because of the acute reaction, they will recover much faster because they are married. However, an "equal" number in the chronic group are also married and they too will get better faster than they would ordinarily do. When comparing the groups, then, the decreased recovery time due to the married cases in the acute group will be cancelled by the decreased recovery time due to the married cases in the chronic group and what will remain is the relatively slight difference favoring the acute patients. Thus, at this stage, the difference in hospitalization time between the two groups will be about the same as it would have been even if a stratum for marital status had been specified.

It is at this point, however, that the trouble begins. The fact that there are some married and some unmarried cases in both groups will make the groups more heterogeneous; that is, within each group, the figures on length of hospitalization will range far more widely than they would have had the stratum been specified so that a sub-sample would contain either all married or all unmarried patients. This increasing range of figures is, of course, associated with increasing variability so that the background against which the relatively slight difference between acute and chronic cases must be evaluated is fluctuating much more grossly than might have been the case. Against such a background, the difference is going to be harder to see and, as pointed out earlier, statistics will take this into account.

Let us emphasize again that the difference between the acute and chronic groups will be about the same whether marital status has or has not been specified as a separate stratum. Now, however, this difference must be shown to be significantly greater than the amount of variability present in order for the difference to be called other than a chance event. Since the uncontrolled presence of married and unmarried cases within all groups has inflated the amount of such variability, the difference between the two groups must now be larger to reach significance than would have been the case had marital status been specified and sampled.

We thus reach the unfortunate possibility that our investigator's design may force him to accept a null-hypothesis that there is no systematic difference between acute and chronic cases where a study with a separate stratum for marital status might have arrived at precisely the opposite conclusion. Finally, of course, it is clear that martial status itself will be overlooked as a variable important in determining length of hospitalization.

It is hoped, then, that the case is clear. Heterogeneity is the rule and homogeneity the exception. Stratification will reduce heterogeneity at the cost of increased complexities in both design and analysis. At what point along this spectrum of heterogeneity-homogeneity the investigator wishes to stop, will depend upon the purpose of the research, the desired precision of the results and the practical aspects of sampling and control.

One point should be made very explicit here. It is not the task of the statistician or the outside consultant on research design to set up such stratification or to decide as to what the strata ought to be. These subcategories are not something to be decided upon by fiat but must be reconsidered with each new experiment and each new design. It is experience with the subject matter itself, acquaintance with the hypotheses under test and particularly the genesis of such hypotheses that helps to make the decision as to the various types of stratification needed and the amount of heterogeneity that is tolerable. These decisions are properly those of the responsible investigator or of the research team involved.

THE SIZE OF SAMPLES

In 1881, John Stuart Mill rather vehemently noted that he would not

> ... waste time in contending against modes of argumentation which no person with the smallest practice in estimating evidence could possibly be betrayed into, which draw conclusions of general application from a single unanalyzed instance (12, p. 261).

Note, however, that Mill used the term

"unanalyzed" and then compare his statement with that made by Egon Brunswik:

> ...there is no place for statistics in a strictly nomothetic discipline. In fact, not even averages from a large number of cases of repeated observations are in order.... If all the relevant conditions are known, or rather if all disturbing influences are eliminated, only one observation is needed to ascertain a general law once and forever (2, p. 266).

These two statements present what appear to be two diametrically opposed points of view.[5] In point of fact, they mark one of the often debated arguments between the experimentally- and clinically-oriented research worker. This paper is not the place for a thorough analysis of the logic behind either Mill's or Brunswik's statements although each is quite true in its own frame of reference. For our purposes, it is sufficient to note, as Brunswik points out, that a "sample" of one is adequate if and only if "...all the relevant conditions are known ...all disturbing influences are eliminated..." It is hoped that no reader will take research on the human being as meeting the above limiting criteria. This being the case, we are forced to say that more than one case is needed and, at once, question why. The answer must be couched in terms of errors and precision of measurement.

Essentially, the logic behind Mill's plea for repeated samplings is that random errors tend to cancel out against each other. This reasoning may be applied to both kinds of experimental sampling procedures: estimation-research and hypothesis-testing.

In estimation-research, the investigator is concerned with making an educated guess as to the actual population parameter by deriving it from a representative sample. Let us suppose he is interested in estimating the mean of a population and (for the sake of simplicity) has drawn a random sample. In the population, the mean is determined by the average of the cases falling above and below it. A random sample would mean that any case in this population has an equal *chance* of being drawn. A random sample does not guarantee that an equal number of cases from each side of the population mean *will* be drawn; it only gives us a good chance of so doing and the more cases we randomly sample, the better our chance will be. Thus, the more cases we draw, the more they will eventually balance out, the more the cases from each side of the mean will cancel each other, and the more closely will the investigator's estimate from the sample approach the mean from the population. As more and more cases are drawn, the discrepancy between estimate and parameter will decrease so that—in the limiting procedure—if all of the population were drawn, the estimate would *be* the parameter and the deviation between the two equal to zero.

In testing hypotheses, the investigator is typically interested in assessing the difference that exists between two groups as a function of some "treatment." This difference must be measured in terms of some kind of response. However, since one is never able to eliminate all of the disturbing influences or specify all of the relevant conditions, every time this measure is taken it will be subject to some degree of error tending to mask or distort it. Without *a priori* knowledge, this error is assumed to be random, *i.e.*, as likely to change the response one way in one subject as it is to change it the other way in a different subject. The logic, then, behind increasing sample size is that these errors will tend to cancel out against each other. Such a self-corrective device then tends to provide the investigator with a more accurate estimate of the true value with which he is concerned.

[5] It should be noted, however, that Brunswik's position is the complete antithesis of the quoted statement. He makes the statement only to show that he believes it completely impossible to fulfill. His basic viewpoint is that "...there can be no truly molar psychology dealing with the physical relationships of the organism with its environment unless it gives up the nomothetic ideal in favor of a thoroughly statistical conception" (2, p. 271).

This formulation should help in substantiating Mill's claim that one needs more than one case to make "... conclusions of general application ..." But, of course, the question remains as to how many more.

Noting, initially, that there is no hard and fast rule to settle this problem, a partial answer may first be adduced by pointing out that when the significance of results is being judged increments in sample size inevitably go along with increments in precision. Because of the reduced effects of variability, a given figure is always judged to be statistically more significant (and/or more precise) as the number of cases on which it is based increases. Therefore, if one hopes for statistical significance, for the accurate and sensitive uncovering of differences, or for the closest estimate of a parameter, one increases sample size. Other things being equal,[6] one is always safer and more accurate with larger samples when utilizing estimation procedures; as long as it meets the criterion for random selection, the bigger the sample, the better the estimate.

Can the same be held for hypothesis-testing? The answer is affirmative with one fundamental qualification. A distinction of importance must first be made between results that are significant and results that are meaningful. Increased sample size will always make for increased precision and sensitivity but it will not do much for the problem of meaningfulness.

An example may clarify this difference. Suppose that a new nutritional scheme is proposed for Army personnel and that the criterion for the scheme's success is an increment in weight. A pre-post measure of weight is taken with the new diet intervening and a mean increment of .25 pounds is seen. Suppose that there are 50 subjects in this sample and that this mean increment does not reach significance. If the sample were now increased to 5,000 subjects—and the mean difference remained exactly the same—it would quite likely be significant. But the weight difference is still only one-quarter of a pound. Despite the statistical significance does this increment have practical significance? Is it meaningful? It can be seen that this is a question which must be answered in terms of each individual piece of research. If the new scheme costs less to use, then certainly the results are highly meaningful. If it costs more but weight is a crucial variable, then it may still be meaningful. But if it costs much more, or if subjects complain about it, or if weight is not important unless it changes markedly, then the results must be dismissed as statistically significant and practically unimportant.

With this qualification, then, it may be repeated that precision and sensitivity are better with more cases whatever the purpose of the research.

It is perhaps painfully clear at this point that this discussion is begging the question of how large a sample one actually needs. The reason is that no specific answer can be given. A rule can be offered which bears, unfortunately, a marked similarity to the old definition of "enough" as being "... less than too much and more than too little." The best answer to the problem of adequate sample size is that: A sample should (if appropriate, be representative of its own population and should) be as small as is sufficiently large (*sic!*) to yield significant results.[7] Perhaps the best summation for

[6] Unfortunately, "other things" are never equal. The time, the money, the practicality of an endeavor must all bear on the size of samples. Even more important (from a theoretical point of view) is the question of the desired precision in the experiment which will be discussed shortly.

[7] Two statistical methods will allow some greater degree of specificity in establishing the size of a sample than does the above statement. The first of these techniques requires some type of "advance" information such as would be available from a pilot study or from previous research. If a prior estimate (for example, the mean and variability) of the responses under a particular experimental condition is available, it is then possible to estimate the size of the sample needed for significant results using this same type of experimental condition again. A straightforward method for computation of sample size by this technique is fully discussed by Edwards (6, pp. 153–155). Other aspects of this problem, although more tech-

this thesis may be presented by quoting E. F. Lindquist:

> ...the aim of the experimenter should usually be, not to provide for the highest possible degree of precision in the estimate, but rather to secure, *with the minimum expenditure of his resources*, whatever degree of precision and freedom from bias is *sufficient* for his purposes.... ...what is involved in "cost" or in what units it may be measured is often very difficult to say. The true cost of an experiment may seldom be satisfactorily described simply in terms of dollars expended. Whenever human subjects are employed, the time, convenience, comfort, and motivation of the subjects are often more important than the time and convenience of the experimenter, but as elements of cost these factors are very difficult to assess or describe quantitatively. It should be clear, however, that the more precise of two experimental designs is not always the one to be preferred. The "cost" at which this precision is obtained is always an important consideration in the choice (10, pp. 4–5).

SOME THEORETICAL AND PRACTICAL CONSIDERATIONS

To this point the discussion has neglected the realistic problems facing the researcher in the behavioral sciences. Large numbers of subjects have been advocated as usually desirable with no consideration of whether they are, in fact, obtainable. It should be made clear, then, that it is not the purpose of this paper to state categorically that large numbers of subjects are so desirable and so necessary as to constitute a *sine qua non* for research. Such conditions are often idealistic ones that are difficult to fulfill and the question must often be posed as to whether a piece of research should be initiated when the size of the sample is very small or even reduced to but one case. This question cannot be answered except as the individual research project is concerned.

Should there arise a unique opportunity to investigate a small group of subjects—or, for that matter, one opportune case—and should the investigator be able, in all scientific honesty, to say that the situation is really unique (and not just an excuse for abandoning the many difficulties inherent in adequate sampling), then certainly it is rigid and pedantic to maintain that the investigation had better not be done at all barring an adequate sample. Too often, the research opportunities in medicine—and particularly in psychiatry and psychosomatic medicine—are not planned but simply happen along. They are, in truth, unique events which if passed up sacrifice the acquisition of important clues for the satisfaction of elegant design. But it must be kept firmly in mind that adequate sampling is still adequate sampling, that rationalization is yet a defense, and that the two are not mutually equivalent. For if a substantial sample *is* to be had and some very few members of this population are christened as unique to save the investigator the time and trouble that go with compiling an adequate sample, then we have substituted error for precision, specificity for generality, and hunch for evidence.

In short, although there may be times when adequate samples are unavailable and inadequate samples are both justifiable and recommendable, one should not imagine that such justification renders the sample now magically adequate. It remains exactly what it was, a matter of expediency and not good design. This inadequacy should dictate extreme caution in the interpretation of whatever results are forthcoming and it is unfortunate that so much of the literature—particularly in the field of psychiatry and psychosomatic medicine—fails to abide by

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nical in nature, are covered in articles by Commins (4) and McHugh (11).

The second technique, rather than requiring prior knowledge, operates during the course of the experiment itself. This is sequential analysis. Here, each subject is examined, in turn, until a point is reached at which a decision can be made whether to accept or reject a null-hypothesis. Although the technique is attractive, it should be emphasized that it is relatively complex to design and should be used with caution. A very useful discussion of this device is afforded by Dixon and Massey (5, Chap. 18).

this simple methodological safeguard: *One does not generalize from an inadequate sample.* This is a simple rule, it is an accurate rule. Too often, however, it seems "more honoured in the breach than in the observance." Some greater degree of attention to its meaning and to the logic behind it might serve to reduce the host of unfounded generalizations that confront the behavioral sciences.

SUMMARY

This paper is concerned with some of the more important theoretical and practical aspects of sampling. The major points have been 1) that facts are discernible from inadequate samples but that generalizations are not; 2) that randomization is an essential aspect of all sampling procedures although capable of being introduced in many different stages of sampling; 3) that all techniques for sampling form some variant on the basic methods of the random and stratified sample; 4) that there are two basic types of investigation, estimation-research and hypothesis-testing, each implying different methods of sampling; 5) that the most important aspect of a population to be considered is its heterogeneity; 6) that heterogeneity may be diminished and homogeneity increased by stratification of population sub-samples with sampling instituted within the strata; 7) that homogeneity increases precision and sensitivity and 8) that increments in sample size do the same but 9) that precision is not a be-all or end-all but something to be evaluated in terms of the efficiency of the research since greater precision always "costs" more; 10) that there are certainly times when research on inadequate samples is justified and recommended but that the inadequacy of the sample must be considered and unfounded generalizations guarded against.

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ADAPTATION AND INFORMATION IN PSYCHIATRY

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The two values generally most highly prized in theoretical statements are parsimony and generality. Of the two notions with which I am principally concerned here, that of adaptation has already demonstrated its usefulness in the field of psychiatry. With this as the central idea, both the parsimony of statement and the generality of reference may be increased through a synthesis of some ideas from the field of communication engineering and information theory (11, 14, 23).

For theoretical purposes, the dynamic component of any on-going process may be described either as a "push" from behind or as a "pull" from in front; the choice is put in the classic metaphor of the donkey motivated by stick or carrot. Psychoanalytic theory, perhaps the most prominent systematic statement in psychiatry at the present time, is a push or drive theory, based on the notion of "instincts" which demand satisfaction. The idea of adaptation, on the other hand, is a "pull" theory, based on the notion of the pursuit of a goal. The teleological orientation of adaptation is shared by information theory.

Ideas of adaptation have a long and prominent history in biology. Claude Bernard (3) formulated as the general goal of physiological processes the preservation of constancy in the internal environment, and in a subsequent development of this idea, Cannon (4) spoke of the preservation of a "steady state" in the organism. The recent work of Selye (16) points out that the physiological processes of the endocrine system can be described as oriented towards the general goal of compensating for stress. In a previous paper in this general area, the writer has discussed in some detail the manner in which these notions illuminate the problem of anxiety in human beings (19). Here I should like to follow out some implications of a synthesis of ideas of adaptation with those of information; the general assumption is that adaptation, as a process which involves a relation between a system and a sub-system, depends upon communication between the two, and that this communication can be described in quantitative terms through the use of the idea of information.

This discussion principally concerns problems of learning in a broad sense. Comments from two contemporary authorities indicate the use made of the idea of adaptation with reference to intelligence. In discussing the relationship of Dewey's work to modern scientific concerns, Condon (5) has said, "He (Dewey) points out clearly that the growth of rational thought processes may be considered as a response to the biological necessity of adaptation to the environment. Its ultimate function, he says, is that of 'prospective control of the environment'. It follows then that 'the function of the intelligence is not that of copying the objects of the environment, but rather of taking account of the way in which more effective and more profitable relations with these objects may be established in the future' ".

Through several decades of work, Piaget (12) has used the idea of adaptation as the central one in his theoretical formulations. In a recent book, *The Psychology of Intelligence*, he says in the introduction:

"Every response, whether it be an act directed towards the outside world or an act internalized as thought, takes the form of an adaptation, or, better, of a re-adaptation. The individual acts only if he experiences a need, *i.e.*, if the equilibrium between

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the environment and the organism is upset, and action tends to re-establish the equilibrium, *i.e.*, to re-adapt the organism (Claparede)."

The teleological orientation of ideas of adaptation link it with modern ideas of communication and control in information theory. Within the past two decades important developments in this field have led to the possibility of quantification where previously it was not feasible. The device through which this has been accomplished is that of establishing quantities in purely relative terms without reference to any absolute unchanging standards. It is the purpose of this paper to discuss in some detail the use of these notions systematically in attempting a further understanding of clinical problems in psychiatry.

ADAPTATION

The basic idea of adaptation is remarkable in its generality, describing a process having any number of possible applications. The time span of adaptive processes may be measured in millions of years in the evolutionary changes in species, or in fractions of seconds, as in the response of the iris to a bright light. It is a neutral idea, referring to a relationship between two participants, (or, more precisely, to a transaction involving two parties) (2). Certain adaptive processes may be described in terms of values as progressive, others as regressive. We generally think of learning to use a language more skilfully as adaptive to living in a social system, but in cases such as that of Robinson Crusoe and a schizophrenic patient on a "back ward," we see as an adaptive process of a regressive nature the deterioration of the ability to talk. This deterioration in the two cases is adaptive to the situation, in a manner analogous to that of the deterioration of eyes in the fish living in total darkness in Mammoth Cave. It is highly characteristic of many psychiatric syndromes that they should be regressive but adaptive, as for instance the celebrated horse-phobia of "Little Hans" which protected the child's relation to his father.

The term adaptation describes a relation involving a larger system and a component sub-system, each of which is open to, and communicates with, the other. When the sub-system is small enough in relation to the larger so that its influence upon the larger system is negligible, the terms "organism" and "environment" used in relation to them are maximally useful; in such a case the smaller system is an example of an "open" system (22).[2]

The boundary separating system from sub-system is a matter of definition for the purpose at hand; this matter is again of great importance clinically, since it is common to find patients defining themselves quite differently in different situations (a clear example of this is the difference in self-definition by the same person when a pedestrian and when an automobile driver). Any given sub-system may be organism in one context and environment in another; for example, a family is environment to its component members, but it is one of many sub-systems in relation to the larger society. Many psychiatric and social problems deal with conflicting tendencies of an adaptive nature; for example, it is in the short run adaptive for a society to reject its deviants, but rejection tends to lead to deterioration on the part of a deviant: the rejected criminal tends to become a member of the prison social system, a "hardened criminal" (20), and the rejected schizophrenic tends to become a "vegetable."

Adaptation is inextricably bound up with

[2] The relation of the two systems to each other is very complicated when they are not too different in extent, as for instance in the case of an animal in an enclosed space: here the excreta of the "organism" may poison the system so that the "environment" will no longer support life. Often in psychiatry we find complications when the social system forming the environment of a patient is too limited in extent; as perhaps the primary example, in this culture, the absolute dependence of an infant upon its mother makes the infant extremely vulnerable to any incompetence on the part of the mother.

processes of communication. Adaptation requires both 1) a sensitivity to changing conditions, and 2) a responsiveness by means of which to meet these changes: sensory "inputs" lead to effector "outputs," a change in the former leads to changes in the latter, and in turn the changed conditions brought about by the effector activity are fed back into the system via new inputs. The openness of the arrangement makes for a continuous circularity which can only be described by artificially simplifying it through the "input-output" device.

In any scientific observation serial systems are involved; in psychiatry we deal with a patient in an adaptive relation to his family, but we deal with this problem in the context of a relation involving patient and therapist, and we evaluate the therapeutic relation in a further system involving therapist and supervisor or consultants.

Sensory processes are concerned with analysis of forms, while effector processes are involved in some type of movement in response to the form. These two processes can be called *form discrimination* and *responsive movement*, and they are basic respectively to the development of intellectual analysis and effective responsiveness.

A beginning analysis of some of the potentialities of this formulation leads to the idea that there are two basic (yes, no) choices in either of these contexts: the perceiving sub-system may (1a) move toward, or (1b), move away from, the form perceived, or it can (2a) act to change the form perceived, or, (2b), act to change its own form. Several or all of these processes may take place in a single complete act, *e.g.*, one perceives food, moves toward it, then moves it toward his gastro-intestinal tract, changes its form by mastication, then perceives a change in his own self awareness from a hungry to a replete state.

This complicated process requires a series of decisions, each of which is signalled to an observer by behavior on the part of the sub-system of interest. With respect to a given reference point (*e.g.*, food in the example above) movement toward is a "yes" decision, movement away from, a "no" decision. Recent developments in the field of communication theory offer a powerful tool which promises much in this area. This tool is the nation of *information* in its technical sense (14, 23).

INFORMATION

The broadest and simplest definition of information is "anything which makes a difference." This definition says nothing whatsoever about the form of the "anything" nor does it in any way specify the "to what" the difference is made. Information is qualified in terms of units called *bits*: one bit of information is that amount which makes it possible to decide between equally probable alternatives. The simplest example of this is the familiar coin experiment: if a coin is tossed into the air the probabilities are equal that it will land heads or tails; it takes, therefore, one bit of information to make a decision as to which of these has actually occurred. Information is conveyed in *messages*: observation of the coin constitutes a message. The message travels along a *channel*: any medium through which a message can be conveyed is a channel for that message. The general nature of the notion of information has been discussed by Rothstein (14) in a paper demonstrating the logical identity of the problems of measurement and communication. There Rothstein discussed various implications for physics and mentioned the possible relevance of the notion for biology.

The unique advantage of the idea of information for psychological purpose lies in its almost paradoxical character of being a dimensionless quantity. Information in this sense says nothing whatsoever about the channel nor about the content of the message, nor does it specify the sort of observation by means of which a decision can

be said to have occurred. The measure of information is completely divorced from any of the usual measures of intensity of stimulation, as, for example, the familiar physical measures of foot-candles or decibels. The passage of a message is judged from its behavioral result as, for example, when a light goes on, it is inferred that a switch has been turned. From the point of view of the information conveyed a trivial or a world-shaking decision carries the same single bit of information if the alternative probabilities are equal prior to the decision.

The notion of adaptation lends itself readily to expression in terms of information. The idea of adaptation implies a goal, and this goal can be stated in general terms as that of maintaining the input of information constant within limits.[3] Living organisms have the potentiality of processing greatly differing amounts of stimulation (expressed in "absolute" terms) to yield similar amounts of information. For example, the visual mechanisms allow significant observations to be made in semi-darkness as well as in brilliant noonday sunshine. When a dazzling light is turned on in the middle of the night, the eye rapidly adapts itself to the new level of light intensity.

A major difference between living and non-living systems is to be found in terms of the limits within which input can be processed. In non-living systems the limits are fixed; for example, a given type of photographic film can be used in relation to a narrow range of intensities of light.[4] Living mechanisms are characterized by variable limits; these limits differ through usage and learning, both acutely and chronically.

The limits within which input can be processed to yield useful information are "thresholds"; the upper and lower thresholds define an "area of toleration." The upper may be called the "threshold of tolerance", the lower that of "indifference" (1). Above the upper threshold, an event is no longer a stimulus but rather a trauma; below the lower, an event is not perceived.

It may be said either that there is a certain ability to tolerate stimulation, or else that there is a certain capacity to process information. The area of tolerance defines the zone in which incoming stimuli can be efficiently matched to an appropriate behavioral response; in informational terms this zone defines the possible inputs or messages which can be received. The area of toleration is equivalent to a capacity, and this capacity corresponds to the notion of a "channel capacity" in communication theory. In living organisms a repetitive input at a tolerable level leads to the development of an increased capacity; if the input ceases or falls below the threshold of indifference the capacity undergoes disuse atrophy. When the input is almost but not quite at the threshold of toleration the capacity tends to increase more rapidly, but when input is very much greater than the channel capacity there is a traumatic effect. Following trauma there tends to be not only a diminution in channel capacity but also some distortion of its function.

Living systems constantly change in relation to experience. Problems that are repeatedly encountered come to be anticipated, and "systems of expectation" are constructed by means of which the balance of a whole is predicted from a small part or

[3] This idea of constancy was early employed by Freud (6).

[4] It is interesting that in order to measure accurately quantities of stimulation, man has to rely upon instruments which are characterized precisely by their nonadaptive character. An "absolute" measure of length, of light, of weight, is a measure comparing an unknown with an unvarying standard. Because of this peculiarity, science has achieved relatively great success in problems which can be thus mechanically reduced to manageable proportions, whereas only recently has it become possible to manage with any degree of preciseness the problems in relative quantification which all animal species have managed operationally and unconsciously now for millions of years. This is a comment on the different orders of the problems of "knowing-how" and "knowing-about."

"cue," as, for instance, one begins to expect a thunderstorm when the sky begins to darken. The system includes a number of potentialities, and in information theory terms it can be called an "ensemble of messages."

The possibility of communication between any two respondents is based upon the possession of a mutually accepted ensemble of messages; put in a less abstract way, we say the two respondents have to speak the same language. In a psychiatric context, it is of greatest importance that such a shared ensemble of messages makes possible both cooperative and competitive relationships. Often it appears that a relationship persists in spite of a great deal of apparent discord because neither respondent has another with whom he shares the same ensemble of messages.

Messages are ranked and ordered in terms of probability of repetition, from highly probable to highly improbable. Here the idea of information is specifically of great value, since the information conveyed by any message is inversely proportional to its probability: an improbable message conveys much information, a probable one little. Much of the work of the psychotherapist consists in discussing with the patient the hierarchy of probabilities built into his personality; for example, it is a common problem in the psychotherapeutic situation that a promotion or other success does not lead to a feeling of pleasure as both patient and therapist would consciously expect it to.

If one takes the series of expected messages in its hierarchy, the whole can be seen to have a systematic character in that certain messages imply certain others with certain qualifications. The whole can be called a "system of expectations," and it is in terms of such a large and complex system of expectations that we all live. We expect confidently that day will break tomorrow, that the office will be there as usual in the morning when we arrive, with the consistent reservations that improbably but possibly, day will not break as expected because of an eclipse of the sun, or that the office will have burned down in the night.

The "prospective control of the environment" depends upon systems of expectation; to a considerable extent the extent of the evolutionary process may be measured in terms of the extension into the future possible in the given species' capacities. Sherrington (21) has pointed out how the evolution of animal species can be traced through a progressive development of the "distance receptors" and their central connections, characterizing the cerebrum as the "head ganglion" of the distance receptors. The general idea of systems of expectation is general enough to cover both functional and structural aspects of animal life, since any structural adaptive modification predicts the environment to which it is adapted: fins predict water, lungs predict air.

With specific reference to psychiatric problems, the notion of a "repetition compulsion" is essentially the same as that of a system of expectation except for the "push" rather than the "pull" dynamics in the former case. We can say either that one is forced to repeat archaic types of behavior, or that one is hampered by expectations which persist in spite of repeated lack of confirmation. In the operations of most psychotherapists, the goal-directed orientation takes precedence in the manner in which the therapist points out to the patient how he repeatedly expects to be treated in a manner which repeatedly proves to be erroneous.

THE DEVELOPMENT OF CAPACITY

Many theories of personality use the early feeding relationship as the model or prototype of human development. To provide himself with food is the initial problem which the newborn has to solve, and the peculiar helplessness of the human infant in this respect has a vast number of con-

sequences. The human infant has to develop even very basic behavioral patterns of form discrimination and responsive movement.

The human newborn gives the impression of avoiding any kind of stimulation; it may be inferred from his behavior that any input is disagreeable, and he reacts with a massive muscular response. This response is quite generalized in regard to both input and to output; it is produced by anything that rouses him, and it affects his whole body. If the mother attending the baby understands this reaction as being the result of hunger and offers to the baby under the proper circumstances a source of food, the baby soon learns the muscular movements (13) which constitute the channel for the acceptance of milk. He soon learns to expect to be relieved, and his preparatory "set" corresponding to the input of information meaning "hunger" consists of a variety of acts and postures which make it possible for him to satisfy himself rapidly when supplied with a full bottle or breast.

Quite early the human infant begins to demonstrate how much one input may be substituted for another. In this process he shows that it is the informational rather than the substantive value of stimulation which is prepotent in human affairs. This first substitution is that of the thumb (or other "suckable") for the nipple: the infant learns that his distress can be eased, at least in part, by the substitution of the readily available sucking sensations for the less readily available taste and smell of milk. To the extent to which this substitution works, the infant has made himself independent of the environment by utilizing one of his own potentialities.

With the development of a system of expectations, the affective component of the process tends to change. The original introduction of the nipple into the mouth of the newborn is frequently or commonly resisted (a negative affective state) for a brief period: the infant has to learn to suck, even though this takes place in a brief time. After the learning has taken place, the infant's adaptive output indicates a positive affective response. For a long time in childhood, the introduction of any new food is met with a negative affective response with rejection; this rejection changes slowly in the case of habitually encountered foods to a tolerance and an eventual active enjoyment. This change in affective component continues to a point later on in life at which a mild hunger is actively pleasant to the individual if the prospect of alleviating it is not distant.

Two processes take place concurrently in the development of tolerance, those of differentiation and of the widening of capacity. By means of his own muscular and visceral responses the infant begins the construction of series of stimulation and responsiveness extended in time, in which each stimulus induces its appropriate response, and the response includes as an integral part an anticipation of the next stimulus (1). Each series represents a skill and the skills developed may be described in classes, *e.g.*, eating, crawling, walking, or dressing oneself. When well developed any series includes a number of different actions smoothly blended into a single unit in which it is impossible to draw a distinct line between the completion of one action and the preparation for the next.

The difference between the infantile mass action response to stimulation and the skilled precise serial response is the essential difference between a non-adapted and an adapted state. The newborn's response to any stimulation is explosive. His behavior is chaotic, and it requires a skilled and dedicated mother to accept the infant's distress as a series of undifferentiated messages rather than as a cause for alarm. The ideal complement to the generalized explosive state of the infant is a reciprocally particularized and skillful system for receiving his messages.

The pattern of adaptation can thus be outlined. A stimulus which at first induces

a generalized response comes to be specifically related to an output; in this process, the stimulus comes to be tolerable. At the same time that toleration is established by adaptation, so is expectation established. The stimulation, at first disorganizing, comes to acquire the character of necessity as it is fitted into the expectation system. When the stimulation becomes necessary, the same process which at first attended the introduction of the stimulation now attends its absence, *i.e.*, a generalized and disorganized output occurs when the expected repetition of the experience is not forthcoming. In the course of growth at the time when a certain experience has to be relinquished, the process follows the same development. The human infant needs first to become accustomed to various sorts of experience, against resistance, which he then must relinquish, again against resistance, to become adapted to others and so on. The serial adaptation process depends to a great extent upon somatic maturation; too early an exposure to certain types of experience may permanently impair the individual's capacity to adapt, and, conversely, if certain experiences are not encountered at a time when the biological mechanisms involved are sufficiently plastic, they may never be integrated satisfactorily. The intensity of the experience leading to successful or unsuccessful adaptation is thus estimated not only in terms of the magnitude of the observed event but also in relation to the readiness of the individual and his needs. Events appearing to a distant observer exactly identical may eventuate in a progression, a fixation or a regression in the development of the individual directly concerned, depending upon his simultaneous developmental state of readiness.

SOME PSYCHIATRIC CONSEQUENCES OF THIS VIEWPOINT

The important distiction between conscious and unconscious can be expressed in more precise terms using the idea of information. An output corresponding to a given input may be observed by the actor himself or by another. Where the action is meaningful to the observer but not to the self, it is said that the motive imputed by the observer is "unconscious" to the self. Much of the interchange between a psychotherapist and a patient consists in remarks designed to convey to the patient the doctor's interpretation of the meaning of the action. As Freud clearly pointed out (8), the "unconscious" action becomes "conscious" when associated with verbal symbols, *i.e.*, when it can be described; in the therapeutic situation the unconscious becomes conscious through its description in the words of the therapist.

When an input is repetitively experienced, there is a strong tendency for the input to become linked to its appropriate output habitually. When this relation is established at a high degree of probability, it persists at a completely unconscious level. After a good deal of experience the habitual problems presented by even a very complex situation can be solved without conscious attention. For example, the first experience of driving in the rush hour traffic of a metropolis may be almost intolerable to the inexperienced driver, whereas after having become familiar with the noise, the speed, and the patterns of control, he may take the same situation for granted, operating his automobile at an unconscious level until some emergency or necessity for unusual decision occurs.

The central concept of modern psychiatry, anxiety, can easily be formulated in terms of information theory (19). Anxiety is the reaction to uncertainty, and the more improbable the message received the more anxious does the recipient become. Freud (7) differentiated between "primary" and "secondary" anxiety: primary anxiety is that chaotic state of disorganization occuring in the newborn as a consequence of a complete inability to process incoming in-

formation. Secondary anxiety occurs when developed methods of processing information are under stress. Freud emphasized that secondary anxiety has the function of a message; as a physiological state having a form similar to that of primary anxiety but in a greatly diminished intensity, secondary anxiety warns human beings of approaching disorder. One of the tasks of the process of maturation is that of developing the detachment required to examine the total situation in an attempt to understand and prevent the danger.

In informational terms, primary anxiety occurs when there are no established categories of response to sensory data. With experience, categories are set up, each of which is related to an ensemble of possible messages of varying probability, from usual to very unusual. Within the category, the intensity of (secondary) anxiety is related to the probability of the message received; a routine message carries almost no information and arouses no anxiety; a somewhat unusual message evokes interest or curiosity; while an extremely unlikely message tends to arouse a good deal of anxiety. The problem of the relation of an unusual message to adaptive capacities was observed repeatedly in a personal experience (10, 18) in interviewing patients suffering from cancer. The message, "You have cancer," is so unexpected that it is regularly disorganizing in its first impact on the systems of expectation and resulted in a depersonalized state. On the other hand, if the patient was not given the eventual opportunity of adapting to this essential knowledge about his future prospects, the eventual result was a disorganization because of inaccurate expectations. In this investigation our conclusion was that the best method of dealing with this overwhelming message was that of allowing the patient to find out the truth in his own time, assuming that his adaptive capacities would in most cases allow him to assimilate the information at an appropriate pace.

It is well known that emotional illness tends to apear at transitional phases in the life of an individual, *e.g.*, at the time of childbirth or losses by death, puberty or the climacteric, marriage, changes in job status, and the like. Each of these changes requires the establishment of a different system of expectations, *i.e.*, the setting up of a different ensemble of possible messages. The problem may be illustrated in one of the early crises of adaptation faced by an individual—that of the Oedipal situation. In his early life, an infant has to make an adaptation essentially only to one person (or type of person), the mother, but at the Oedipal phase he has to learn to adapt to two parents who have not only a relation to him but also a relation to each other. The problem becomes greatly exaggerated because of the three-person interactional system which must be established. If one person, A, has a relation to B such that there are n possibilities of meaningful interaction, and if then another person, C, brings into the situation an equal number of possibilities, the resultant system is one in which the possibilities are multiplied rather than added (23). The ensemble of messages which must be coped with in the latter case is not of the value $2n$, but rather of the value, n^2. The expansion of a channel from a capacity of n to one of n^2 is a problem of great moment.

Head (10), in his *Studies in Neurology*, asserted that the function of the thalamus was to signal "a difference has occurred" without specifying the type of difference. If this be a correct statement of the function of this organ, it fits precisely into the idea of a basic yes-no signalling system as a way-station in the path of sensation toward the cortex. Presumably the thalamus would have the function (perhaps in connection with the nearby alerting systems) of announcing "a difference" and of directing attention of the exteroceptive sensory organs and their cortical "head-ganglion" to the examination of the situation to de-

fine the type of difference which has occurred.

Two possibilities of a breakdown in the personality can be defined in these terms. It is an "open system," characterized by steady states of related input and output. The primal state of the human being seems to be one of expecting minimal input, but there is a rapid development of a state in which a lower limit of input is essential in order to avoid "disuse atrophy" of the given channel. So long as the input then remains within the appropriate limits, there is relative stability. If the input falls below the lower limit, the system is threatened by "running down" into entropic disorder; on the other hand, if the input exceeds the upper limit, the system is threatened with an explosion. These two types of possible disaster to the system are related to two general types of "defense mechanism" in human affairs. Of these, the "hysterical" type is aimed at reducing the input of afferent stimulation, as for example, in hysterical blindness; the "obsessional" type seeks to fit a novel type of input to a familiar type of output, as, for example, when the recipient of bad news does a spring house-cleaning to "get her mind off it."

With the use of these ideas, it is possible to suggest analogies between the epistemological processes of human beings and computing machines (15). One process is "digital" in type, using principally the exteroceptive modalities and their cortical connections; it operates to to make distinctions on a yes-no basis. The primary method is one of serial dichotomization, *i.e.*, A and not-A, with the subsequent categorization of not-A into B and not-B.

In a discussion of the function of the cerebral cortex published in 1954, Gooddy and Reinhold (9) state in their summary, "We may therefore conclude that the cortex behaves in much the same way as the 'mechanical brain' which should thus be more correctly described as the 'mechanical cortex'. The cortex selects, abstracts, and integrates the motion patterns of cerebral activity. It is perhaps reasonable to assume that the more active the cortex in any one brain, the better will that brain resolve and integrate its activity, resulting in superior powers of judgment of sensory relationship, memory, symbol use, and abstract thinking."

The other type utilizes the whole body as a "sounding board": an input is matched automatically to an appropriate (but minimized) output, and the resultant complex set of data is analyzed by analogy to former situations and in terms of intensity. The judgment apears as "X is making me feel the way that Y used to." The ability to observe one's own implicit states of response is specifically a human potentiality and develops late. Much of "insight psychotherapy" is a technique designed to encourage this type of self-observation. The rule of free association operates to minimize the acting-out of one's habitual responses and to encourage the simultaneous inhibition and observation of these. The observation of these inhibited states of action leads to the judgment that an "emotional state" is present, either in oneself or in someone else. Generally it is much easier to recognize the significance of an emotional state in another person than in oneself. The process of "objectification" in relation to oneself which constitutes so large a part of growing up is to a considerable degree the development of an ability to evaluate one's own states of potential behavior in a manner similar to that which we habitually evaluate them in others.

SUMMARY

This paper has briefly explored the possibilities of formulation in psychiatry in the terms of information theory. The concept of adaptation forms a bridge from psychiatry to communication theory. Certain crucial differences in the behavior of living and non-living communication systems are

noted. Applications of these ideas to certain clinical problems are suggested.

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PSYCHOLOGICAL STUDIES OF KORSAKOFF'S PSYCHOSIS: III. CONCEPT FORMATION[1]

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In experimental investigations (10, 11) of the perceptual functions characteristic of Korsakoff's syndrome, it was observed that patients with this disease are excessively dependent on the immediate sensory input, in the sense of being virtually unable to detach themselves from it by means of imagery or to change their orientation towards it. They also appeared to be deficient in the sequential integration of concepts. These perceptual defects, of course, severely impair their capacity to form concepts, a process aptly described in terms of testing, checking, and retesting hypotheses. Pick (8) and Gruenthal (4), studying the thought processes of the Korsakoff patient, attributed his cognitive defects to an impairment in the processes of checking ideas or in shifting from one train of thought to another. However, they gave no consideration to any possible underlying perceptual disturbances. Our own observations about the severe and extensive perceptual anomalies characteristic of this syndrome gave rise to the question as to whether the known deficiencies of these patients at the level of concept formation are derivative, *i.e.* entirely accounted for by their perceptual dysfunctioning, or additional to the latter. If they should not be secondary effects, a further question arises: do the Korsakoff patients' impairments at the perceptual and conceptual levels indicate disturbances in the same psychological processes? The purpose of the present report is to present the findings of some experimental procedures designed to answer these questions.

SUBJECTS

The experimental sample for this research consisted of 24 patients of the Boston State Hospital, eight men and sixteen women diagnosed by the neurologist members of our research team as having Korsakoff's psychosis (13), who had been hospitalized for periods ranging from 12 months to over 12 years. At the time of our study these patients were all in the chronic phase of the disease, though several of them had been in observation from its onset with the acute neurological and psychological symptoms of Wernicke's encephalopathy. In age they ranged between 31 and 74 years (mean = 53.3, SD = 9.3), in IQ between 84 and 114 (mean = 103.4, SD = 8.9), as measured by three verbal sub-tests of the Wechsler Bellevue Intelligence Scale (14): comprehension, vocabulary, similarities. These 24 patients, all of whom had a history of alcoholism, formed about three-fifths of a larger sample with similar records, from which they were selected for being relatively least damaged in their cognitive functioning. The remainder were too severely deteriorated to follow the instructions of the experimental procedure, and their impairment was too general to be instructive for studying the very specific psychological disturbance characteristic of Korsakoff psychosis.

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Experimental samples for the various procedures consisted of 20 to 23 drawn from these 24 subjects.

Control samples were recruited from two sources. The first (control group I) consisted of outpatients receiving psychotherapy at the Alcoholism Clinic of the Massachusetts General Hospital, 16 men and eight women chosen for sharing with the Korsakoff patients a history of alcoholism, and having no alcohol in their system at the time of testing, since abstention was a condition of treatment in the clinic. None of these subjects was known to suffer from any disease, neurological or other. The second control group (II) consisted of neurological inpatients at the Lemuel Shattuck Hospital, Boston, four men and four women who had been hospitalized for six months or longer. They were chosen for sharing with the experimental group the experience of prolonged hospitalization, and for neither having a record of alcoholism nor of brain damage. In age neither control group differed significantly from the Korsakoff group, their means being 48.3 ($SD = 10.4$) and 49.0 ($SD = 8.7$); in IQ estimated from the three subtests of the Wechsler Bellevue Scale the difference was significant for the alcoholic out-patient group with a mean of 114.0 ($SD = 12.8$), but not for the neurological patient group with a mean of 103.5 ($SD = 9.8$).

METHODS AND RESULTS

One or more experiments were conducted to investigate the performance of the Korsakoff patients, and to compare them with the control groups, in four aspects of concept formation. These four aspects were width of categorization, classification or sorting, discovery of criterial attribute, and recognition of sequential concepts. Significance of differences between mean group scores were determined by t test or, in the absence of homogeneity of variances, by Mosteller and Bush's (7) version of the Mann-Whitney test for ranked data which gives normal deviates (z scores).

WIDTH OF CATEGORIZATION

Our study of perceptual functioning in Korsakoff patients demonstrated that their range of identification or span of generalization tends to be wider than that of the controls; consequently, it would be expected that, as a result of ignoring certain finer cues of distinction, their conceptual categories too are more comprehensive. Yet are their looser concepts entirely due to perceptual defects? The hypothesis that they are was tested by means of the material shown in Figures 1 and 2. Each of these figures was presented on a white card 3 x 5 in., those on top of the two series being the test models. The subject was shown the model, first with the instruction that it was a *PAM*—a word meaningless otherwise, but in this situation defined by the properties of the design displayed—and that his task was to say this word each time he saw a *PAM* among the cards he would be shown. The instruction was given in words suitable and in sufficient detail to be comprehended by the subject. If he then asked for a closer definition of *PAM*, *i.e.* whether size, color, or certain details of the design mattered, he was advised that this was for him to decide. After he had thoroughly inspected the model, he was shown the test cards one by one, in the order of the illustration, except that the model appeared in positions 2, 6, 7, 11, 13, 22, and 30. The test series therefore consisted of altogether 40 items, and alternate subjects were given the series presented in Figure 1 or in Figure 2. Their responses were "*PAM*" or "not *PAM*", intermediate or qualifying definitions were not allowed. No time limit was set and none was needed since responses were almost always instantaneous and rarely required as much as 20 seconds. Throughout this test the model was displayed in front of the subject.

Second, the same procedure was followed with the alternate series of cards, except that a different nonsense syllable, *BIX*, was attached to the model, and that this card was concealed from the subject, who was not

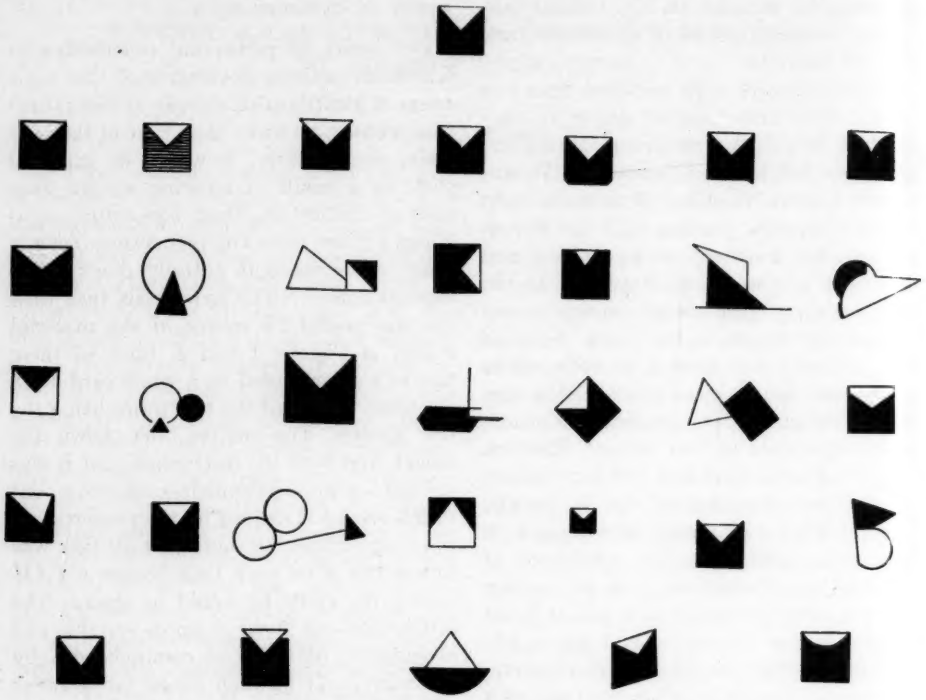

FIG. 1. Figures used in categorization task; Series I.

allowed to refer to it after the test had begun. A third and fourth test repeated the procedures of the first two, using the same material with one difference: in these tests the subject's task was to identify those figures which were exactly the same as the model. Absolute identity was rigorously defined in the instructions given, with specific mention of size, shape, shading, position, and "any other detail."

Two classes of scores were obtained under each of the four experimental conditions: number of correct recognition, and width of categorization, *i.e.* instances of identifying with the model figures which are not the same in every respect (*e.g.* in direction, size, or shading as well as in design). Differences between the two series of cards were statistically not significant (at the five per cent level of probability). Mean scores under the two conditions of presentation (model in sight and concealed), and under the two instructions (defining the class of *BIX/PAM* and testing perception of identity) are presented in Tables 1 and 2. Korsakoff patients and controls did not differ significantly in correct recognition of the model, no matter whether this was in view or not, whether the task was categorization or perception of identity. They did differ significantly in width of categorization or number of cards called *PAM* (*BIX*), whether the criterion was in sight or not; however, the errors in perceptual identification were not significantly different in the two groups. Another statistical way of expressing these findings is by analysis of variance on the Korsakoff group's scores, one variable being the instruction (class or same), the other the presence or absence of the model. The mean frequency of incorrect equation with the model is significantly lower ($F = 97.9$,

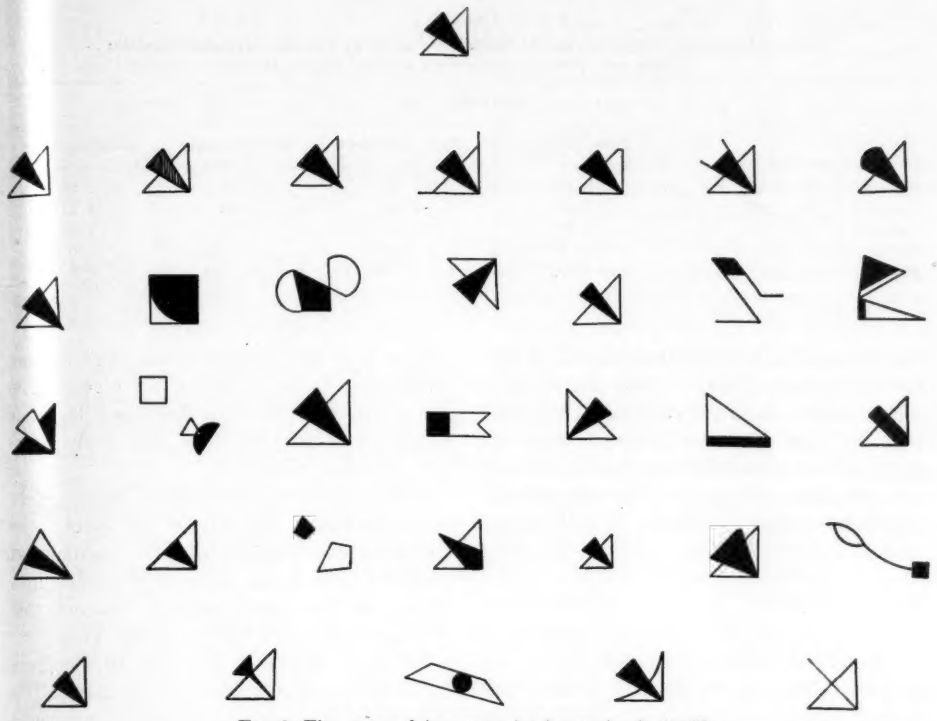

FIG. 2. Figures used in categorization task: Series II.

$p < .01$), than mean width of categorization; however, the presence or absence of the model does not significantly influence either. The means of correct perceptual identification and of classification under the category of *BIX* or *PAM* differ significantly ($F = 11.2$, $p < .01$) according to visibility or concealment of the model but not with the type of task, conceptual or perceptual.

These findings would suggest that bluntness of perceptual discrimination makes a negligible contribution to width of categorization in Korsakoff psychosis, and that defective memory makes none whatever. Nevertheless, categories are wider and more loosely defined, and are so because patients with this disease ignore certain defining attributes in categorization, even though they are capable of observing them under conditions of perceptual discrimination. In some instances they were correct in ignoring these, *e.g.* absolute size, spatial orientation of the design; with some other figures—*e.g.* contour lines carried beyond the point of intersection, or corner cut off—their extension of the category would suggest a lack of perceptual discrimination, except for the fact that under instructions of identity these cues were not ignored. It would appear, therefore, that an attitude of reasonable accuracy in perceptual discrimination can be elicited from Korsakoff patients, but in

TABLE 1

Mean Correct Recognition of Standard Figure

Instruction	BIX/PAM		Same	
	Model present	Model concealed	Model present	Model concealed
Korsakoff (N = 20)	7.00	6.20	6.70	5.65
Control I (N = 16)	6.75	6.81	6.69	6.56
Control II (N = 8)	7.00	6.88	6.62	6.50

TABLE 2

Mean Range of Categorization of Standard Figure by Positive Response to Other Cards, and Tests of Difference between Group Means

Instruction	BIX/PAM		Same	
	Model present	Model concealed	Model present	Model concealed
Korsakoff (N = 20)	15.40	16.00	2.10	2.65
Control I (N = 16)	2.44	2.31	1.62	2.19
Control II (N = 8)	2.25	2.38	1.88	2.38
Kors.—Contr. I	$z = 5.15^{**}$	$z = 7.42^{**}$	$t = 0.65$	$t = 0.54$
Kors.—Contr. II	$z = 2.44^{*}$	$z = 3.70^{*}$	$t = 0.24$	$t = 0.25$

** $p < .01$ * $p < .05$

the absence of such instructions they tend towards greater laxity, which manifests itself in their concept formation. This tendency is even more pronounced when the material submitted for categorization does not offer such simple and obvious cues for classification as the figures in this experiment.

CLASSIFICATION

Heterogeneous selection. An example of the Korsakoff patient's categorizing of objects with less clearly defined criteria was provided by a sorting task. The following 36 objects were placed before the subject: two paper squares, red and blue, 3 x 3 in., two paper isosceles triangles, red and green, 5 x 2¾ in., three paper circles, red, blue and black, 3½ in. dia.; a green square and a red circle of the same size made of sponge rubber; a red elliptical shape in paper and a grey one in cardboard; a green triangle, a red circle, and a yellow square from the Weigl-Goldstein-Scheerer set described below; three square blocks of unpainted wood with bases 1½ x 1½ in., and heights of ½, ¾, and 1¼ in., and a triangular shaped block made of the same material; ten colored wooden blocks, two each white, red, blue, green, and black, some square, others round or triangular in cross section, and all one in. in height; a hard rubber plug; a sponge rubber plug; a rubber washer; a cylindrical electric battery; five steel objects: a nail, a bolt, a washer, a plug, and a paper clip. The subject's task was to sort these objects into groups, no matter how many, so that all that belonged together would be in one pile, those that were different in other piles.

This task, though not always immediately comprehensible, proved to be within the performing capacities of all experimental subjects. The mean number of groups into which 20 Korsakoff patients ordered this material was 12.75 ($SD = 4.80$). This compares with means of 8.12 for 16 alcoholic and 8.62 for eight neurological controls. The differences between the Korsakoff and control means are significant ($z = 3.15$, $p < .01$; $t = 2.07$, $p < .05$) and provide another example of the looser concept formation characteristic of Korsakoff's psychosis. A defectiveness in categorization is also demonstrated by a qualitative analysis, for eleven of the twenty Korsakoff patients sorted the objects without applying any clearcut principle of classification, their criterion being at best a mixed or heterogeneous one. Of the remaining nine, four each sorted according to material or shape and one according to color. In the control groups the breakdown shows eleven instances of shape, seven of material, two of color, and four of composite criteria of sorting. Furthermore, after completion of the sorting task, when the subjects were asked to rearrange the objects in a different way, but still so that those belonging together would be in one pile, only six members of the

TABLE 3

Categorization on the Weigl-Goldstein-Scheerer Task

	Korsakoff (N = 20)		Control I (N = 18)		Control II (N = 8)	
	Discovers principle	Recognizes principle	Discovers principle	Recognizes principle	Discovers principle	Recognizes principle
Both criteria	10	17	17	18	8	8
One criterion only	9	2	1			
Neither	1	1				

Korsakoff group were able to adopt a different principle of classification (two each using material, shape, and color), as compared with all but two of the controls. On a second sorting four control subjects followed a mixed principle of classification. These observations were also confirmed by the more familiar sorting task of the Weigl-Goldstein-Scheerer test.

Weigl-Goldstein-Scheerer color-form sorting test. This test, described in detail by Goldstein and Scheerer (3), consists of twelve flat blocks made of plastic, which divide into three equal groups according to shape (square, triangle, and circle), or four equal groups according to color (blue, green, red, and yellow). Our subjects were presented the blocks simultaneously in random assortment, and were instructed to put those together which they thought belonged together. On successful completion of the sorting task, according to either of the two appropriate criteria, they were asked to regroup the blocks differently. If, in three minutes, a subject failed either in the first or second sorting task, the experimenter demonstrated the principle of classification which he then asked the subject to define. Thus it was possible to score separately the discovery of one or both criteria, and recognition with or without spontaneous discovery. Frequencies within the two samples are listed in Table 3. Recognition of the principles of sorting is clearly not too difficult a task for the Korsakoff patients, and does not distinguish them from the controls; neither does discovery of one appropriate criterion. Discovery of both criteria, however, does yield a difference (control group I: chi sq. = 8.25, $p < .01$; control group II, $p < .015$ according to Latscha's [6] tables for the Fisher exact probability test), which suggests that the Korsakoff group differs in ability to shift from one principle of classification to another. Shape is the criterion they are more likely to use, this principle being the first adopted by seven subjects who were able to shift, and chosen by another seven who could not. In this respect there is no difference between the experimental and control groups; for 18 of the latter began by sorting according to shape, and only five according to color, while three presented both criteria simultaneously. (None of the subjects in either group was color blind.)

DISCOVERY OF CRITICAL ATTRIBUTES

Colored blocks. The three tasks described above each entail concept formation, but in the first two the concept has not been previously defined, so that the subject's performance cannot be evaluated as right or wrong, but merely as narrow or wide, tight or loose. In the third task listed the experimental material allowed for only two correct criteria of categorization, and these were so obvious that the probability of failure to recognize at least one was practically *nil.* Failure to recognize both was taken to be symptomatic of an incapacity to shift criteria rather than of an inability to arrive at a concept. The typical example of concept formation, however, demands initial flexibility in adopting criteria. It is not a matter of indifference as to whether color, shape or texture is chosen as the criterion, since only one of these will serve in discovering the pre-established concept. Unless the appropriate criterion is first selected by chance, it is necessary to switch to others. An experimental analogue to this

situation was produced by means of sixty colored blocks, no two of which were identical in respect of all four of their differentiating attributes.[3] Half the blocks were one in., the other half two in. in height, and within these two groups they differed in any one or more of the following characteristics: color (blue, black, green, red, white); shape (circle, square, or triangle in cross section); size of base (three values each for the square and triangular, four for the circular blocks). The subject was told that some of the colored blocks he would see were *VEC*, an otherwise meaningless word which in this instance was the name given by the experimenter to one group of the blocks. All those that were *VEC* had something in common, which distinguished them from all those that were not *VEC*. The subject's task was to discover what was a *VEC*, or what characteristic made a *VEC*. He would do this by being presented with a number of the blocks, one at a time, and by saying whether each of these was a *VEC* or was not one. The experimenter would always tell him whether his guess was right or wrong, and so, as he went along, he would discover what was meant by a *VEC*. The objects were presented in a prearranged order, in front of a screen so that not more than one block was visible to the subject at a time. He could, accordingly, select any of the four differentiating attributes as defining a *VEC*, and switch to another if his choice was incorrect. According to the design, height was the criterial attribute, all two in. tall blocks being *VEC*. The order of presentation provided for adequate opportunities to test the various hypotheses, by forestalling runs of chance successes; it allowed for at most four consecutive correct responses on the basis of any but the true criterion. After each response, the subject also had to explain his answer. The experiment was concluded by a run of eight consecutive correct responses of *VEC* and *not-VEC*, with a subsequent definition of the concept, or with the sixtieth block.

None of the 21 Korsakoff patients succeeded in discovering the attribute which defined a *VEC*; 16 out of 17 in control group I, seven out of eight in control group II did so after a mean inspection of 10.5 and 6.86 blocks prior to the test series of eight. No subject discovered the correct criterion immediately, shape typically being the first choice; their modal shift of hypothesis preceding solution was two. Thirteen members of the Korsakoff sample discovered the concept when four otherwise different blocks representing each of the two classes, *i.e.* one and two in. tall pieces, were shown them simultaneously, the one group as characteristic of *VEC*, the other of those that were not *VEC*. After this demonstration these subjects succeeded in responding correctly to other blocks shown them singly; another four could do so only after the criterial attribute was verbally defined for them, and yet another four failed even though they had received this explicit advice. All but one of the Korsakoff group attempted solution by shape, and seven of these also tried one or more other reasonable hypotheses, simple or composite, such as bulk or shape combined with color. Four patients considered height, but none held to this criterion, even though it evidently paid off; only one chose color, and none selected size. One experimental subject dealt with four hypotheses, four with three, and two with one alternative; one proceeded without any recognizable criterion. Many of the hypotheses by shape were defined in somewhat roundabout terms, such as "uniform top and bottom"; "same on all sides." There was also a remarkable tendency to employ entirely irrelevant utilitarian considerations, *e.g.* "this could be a doorstep, it is not a *VEC*"; "it does not look like a *VEC*, rather like a hat"; or "serviceable for building a house"; or simply "useful"; "not usable." The task itself elicited some resentment on account of its pointlessness, or the meaninglessness of

[3] The experimental method is described in greater detail in (9).

the term *VEC*. Nonetheless, all the subjects seemed to grasp the instructions sufficiently well to proceed with the experiment, even though occasional comments on their problem solving processes were far from enlightening on the strategy of concept formation, *e.g.* "does not look like that which was a *VEC*"; "different from last one"; "same as the first"; "odd looking"; "I've seen things like that."

Halstead's Category Test. The Korsakoff patients' rigidity in hypothesis formation, their perseveration with criteria which were quite manifestly irrelevant, was no doubt the reason why they performed so poorly on Halstead's (5) Category Test. This test of abstraction and generalization employs a film projector, and presents the subject with a series of 208 geometrical figures and designs, each of which forms a multiple choice situation. The subject's task is to press down one of four keys, the one suggested by the figure, or, more precisely, indicated by a principle which the figure—as well as other figures of the series—illustrates. Of seven series, the first consists of eight Roman numerals, the second of twenty patterns, each composed of one, two, three, or four items. The Korsakoff patients responded to these tasks quite successfully, making no more errors than would occur by chance. However, on the following five series which require the abstraction of more complex criteria (position of the odd object, number of missing quadrant, fraction of the whole figure) they failed completely, their errors varying between 46 and 76 per cent, the latter being about equal to chance performance in a four choice situation. Over the entire test, each of the 20 Korsakoff patients scored well above 50 errors, Halstead's norm for brain damage; the range was 75 to 135; mean = 105, SD = 18.3.

SEQUENTIAL CONCEPT FORMATION

Concepts formed in the temporal dimension, *e.g.* the code of a combination lock or the rules of a ceremonial procedure, would be particularly difficult for individuals who cannot recall what they have done an instant earlier. Such temporal concepts seem to pose difficulties even to people whose memory otherwise functions quite satisfactorily, and who therefore handle such situations by rote learning or magic formulae. The application of the formula, however, may create yet further difficulties, a circumstance more likely to be observed in neuropathological than in normal cases.

The experimental task set to our Korsakoff patients and control subjects required first the discovery and then the application of a sequential concept. Those who failed in the task of discovery were tested on its application only. The material consisted of ordinary playing cards, two decks less the aces, which were arranged in a pattern of *B-B-R-B-R-R*, *B* meaning black, *i.e.* spades and clubs, *R* red, *i.e.* hearts and diamonds. All subjects were familiar with playing cards and knew the four suits. They were told that in this experiment they only had to distinguish red from black cards, their task being to discover in what order the cards had been piled. They were assured that the cards had been ordered according to a fairly simple pattern, and that they would be able to discover it by watching as the experimenter turned the cards face up one by one. They were instructed to predict for each upturned card whether it would be black or red. The experiment proceeded at the subject's rate of performance over 16 sequences of the pattern, unless he predicted sooner at least three consecutive sequences, *i.e.* 18 cards, correctly. If he failed to discover the pattern in 16 sequences, the experimenter told him what it was, demonstrated it with four sequences of cards, made the subject repeat it several times, and then resumed the procedure. If the subject now could predict three sequences without error he was interrupted half-way through a sequence with some such comment as "You see, you can do this all right," and then the task was resumed; further successful per-

formance was interrupted once more. If the subject failed to reach the criterion in 16 sequences he was given a third trial. The sequential pattern was explained to him again, and a model of it (a single sequence if sufficient, otherwise three sequences) was left in front of him, and the same procedure followed as in the second trial. Finally, the subject was asked to reconstruct the pattern from randomly assorted cards.

Two of the 20 Korsakoff patients succeeded in solving the problem, one after two, the other after five sequences. Another two managed to perform the task on second trial, *i.e.* after being shown the pattern, but only one maintained his performance after interruption. On third trial every one could follow the pattern displayed in front of him, but only four managed to resume it after interruption, and only one—the same subject who did not quite make the second trial—continued to perform accurately after removal of the model. Some of the others also continued successfully after losing sight of the model, through as many as six sequences, but then lost the concept and could not recover it. In several instances a model of a single sequence proved of little help, but one of three sequences did enable the subject to make a good start, though not to resume after interruption. Successful resumption, when it did occur, was almost never immediate, but delayed by a few errors. In reconstructing the pattern only those three subjects proved successful who had either discovered it for themselves, or applied it after interruption in the second trial. Eight members of control group I correctly applied the pattern in the first trial, another eight applied it correctly in the second after interruption; one only had to be given a third trial, in which he performed well after interruption. All succeeded in the reconstruction test. Five members of control group II succeeded on the first, and three on the second trial.

Only one of the Korsakoff patients made no attempt to formulate hypotheses to discover the pattern, and treated the experiment as a gambling situation; several others performed in this way, but only after first making some attempt at solving the problem. The Korsakoff patients either volunteered to or were asked to assess their performance after the first trial, and tended grossly to misjudge it, believing they had done well when a considerable majority of their predictions had been incorrect, or thinking they had lost heavily when most of their responses had been right, though not because they had arrived at the appropriate concept. Their principal difficulty was in conceptualizing a combination of single and double alternation; many of their errors tended to be either due to an incorrect application of double alternation throughout (*BBRRBBRR*) or of a two-one sequence (*BBRBBR*). Correct recall of the model was itself no more effective in assuring successful performance than its display; several members of the Korsakoff group remembered it well into the second trial, yet were unable to make the requisite number of correct predictions on single cards. Some of them gratuitously remarked in the first trial that there were never more than two of a color in a run, yet would call out "black" immediately following two black cards. It is an interesting finding that, in addition to their impairment in forming sequential concepts, Korsakoff patients appear to have further difficulties in applying such concepts, though they have grasped and memorized them.

The same deficiency was observed in another and simpler test of sequential concept formation. As in the previous experiment, the subject's task was prediction. The material consisted of cards presenting such simple figures as small and large circles, double circles, triangles or squares, drawn on a plain white ground in India ink. The tasks our Korsakoff patients could attempt all required three consecutive correct predictions of the large circle before it was turned over. The decks were sorted on the following prin-

ciples: (1) a varying number of cards with a single small circle, followed by one double circle, one single circle, and a large circle; (2) a varying number of blank cards followed by a small circle, two blank cards, and a large circle; (3) a varying number of blank cards, among which one square and one triangle was placed each time in a different position, and the large circle immediately followed the second of these figures. No control group was used in this experiment. Most of the 21 Korsakoff patients succeeded in solving and applying the concept in the first series, about half their number also managed the second, and seven the third. Success in applying the principle, after being told what it was, also diminished with increasing difficulty of the concept. There were several subjects, the largest number in the second series, who guessed the principle correctly, verbalized it, but failed to apply it.

A third, and very easy, test in sequential concept formation was also built into playing cards. Two decks were sorted in sequences of diamond-spade-club-heart; the cards were turned over one by one, and the subject's task was to predict the suit of each. All but three of the 20 Korsakoff patients discovered the pattern, although six of these subjects were not always successful in applying it. All controls (19 outpatients and eight hospitalized subjects) discovered and successfully applied the principle. Mean runs preceding success were 8.80 for the Korsakoff group, 2.84 and 3.50 for the control groups ($z = 4.09$, $p < .01$ and $t = 2.70$, $p < .05$).

DISCUSSION

Korsakoff patients are known to have a very limited capacity in handling novel information, and to be confused and inadequate in unfamiliar situations. Their memory impairment would not fully account for these defects, and often it is precisely the intrusion of irrelevant memories, inappropriate concepts or principles of categorization which seem to prevent efficient behavior. One of the reasons for the Korsakoff patients' deficiency in applying appropriate concepts or categories is their perceptual impairment; their inability to observe certain distinctive cues, or to shift their perceptual attitude. While admitting that the distinction between perceptual and conceptual processes is often debatable, in the light of the experiments here reported it can hardly be questioned that some inadequacies in Korsakoff patients faced with tasks in concept formation are additional to their perceptual defects.

In the first place, Korsakoff patients tend to form wide categories of conceptualization, with boundaries that are looser than would be expected in the light of their perceptual impairment alone. Second, they show deficiencies in discovering attributes or in establishing criteria for classification. This second defect may, in part at least, explain their looser categorization. These patients are predisposed towards certain principles of classification, *e.g.* shape or utility, and if those prove inappropriate they are less prone to adopt another attribute for the criterion than are the controls. The extent to which this inflexibility of set results in a concrete attitude will be discussed below. Third, the conceptual functioning of the Korsakoff patients is particularly severely impaired in the face of sequential problems, when their task is to solve or merely to learn concepts formed in the temporal dimension. The larger number of successes they scored on the sequential patterns than with the colored blocks should not be taken as evidence to the contrary, for in sheer measure of difficulty the latter was at a considerably higher level; no attempt was made to present the more elaborate sequential concept tasks to the Korsakoff group.

The question has been raised whether the defects observed in the Korsakoff group in the experiments on concept formation illustrate Goldstein's (2) principle of an attitude

toward the concrete, a lower level in cognitive functioning which he regards as characteristic of brain damaged patients. Goldstein's composite definition which distinguishes the abstract from the concrete attitude does indeed include several cognitive processes in which our experiments have shown the Korsakoff group to be deficient, more particularly the capacity to assume or change mental sets and to keep in mind various aspects of a situation. Some of our findings could be quoted more directly in support of a concrete attitude, notably several of the responses the Korsakoff patients gave to the colored blocks. A global construct of a specific attitude with a unidimensional scale from the abstract to the concrete pole is, however, not entirely consistent with the empirical observations. A tendency to adopt a concrete attitude, if present at all in the Korsakoff patients, is certainly not as marked as their orientational inflexibility; their failure to switch from a concrete to the abstract aspects of an object or situation does not imply a general incapacity to assume an abstract attitude. The conclusion suggested by this present study is similar to the one advanced (12) in face of Buerger-Prinz and Kaila's (1) view, reminiscent of Goldstein's, that the defect in the amnesic syndrome is an inability to take in the total situation. Korsakoff patients change their perceptual sets with greater difficulty than do control subjects, and since their first set is likely to be directed to a detail or to a concrete aspect of an object or situation, they may fail subsequently to reorient themselves so as to take in other aspects of it, whether abstract or concrete.

Regarding the specific implications of the general attitude considered, it should be noted that both Goldstein's own test and Halstead's categorization task produced negative evidence for the argument that Korsakoff patients are unable to abstract common attributes from concrete instances. In the former, several of the patients failed when expected to shift spontaneously from one abstraction to another, but not in arriving at a first abstraction; in the latter they were defeated only by the complexity of the criterion for abstraction. In other experiments they proved quite able to list instances of a class concept, such as names of flowers, they could also solve simple arithmetical problems, and managed without any apparent effort to handle familiar objects in other than their customary use, *e.g.* to categorize playing cards into red and black rather than into the four suits. The sequential concept task, while pointing out yet another source of the Korsakoff patients' defective functioning, provided some additional argument against the hypothesis of their concrete attitude. It will be recalled that several patients could not *apply* the principle consistently even though they had previously grasped it—whether having discovered it for themselves or having watched its demonstration by the experimenter. Behavior in accordance with a concrete attitude would have allowed for a successful application of the concept but not for its correct verbalization. Though the failure characteristic of the Korsakoff group was the exact opposite of this, it nevertheless confirmed one of Goldstein's observations concerning the brain damaged patients' disabilities. The deficiency was clearly not due simply to the subject's forgetting the concept after his comprehension of it, for in several instances he could verbalize it correctly long after he had ceased to apply it effectively, and in other instances he could put it into words without ever being able successfully to apply it. Application, of course, requires a division of his attention between watching the material presented and recalling the principle it illustrates, a continuous shifting of his orientation while checking the fragmentary information of the moment against the image of the whole.

The limitations observed in the Korsakoff patients engaged on the various experimental tasks in concept formation quite obviously reflect a very inadequate structuring

of the environment and of their experiences. The specific defects demonstrated in this report may indeed not be the source of the Korsakoff patients' generally disturbed functioning, yet they are not entirely derivative. However, the foregoing analysis of the Korsakoff group's defects in concept formation has demonstrated not only that these are superimposed on their perceptual defects, but has also pointed to some parallels between the two sets of impairment, which in turn would suggest more basic constructs common to perceptual and conceptual processes. Loose conceptual categorization has a parallel in wider perceptual generalization, rigid adherence to preconceived criteria in dependence on the sensory input, inability to form sequential concepts in failure to integrate sequential percepts. Set rigidity, an incapacity to alter attitudes in the face of evidence to which they could be—though in fact are not—appropriate, is a characteristic common to the Korsakoff patients' perceptual and conceptual functioning. In both perceptual and conceptual tasks their failure suggests an inability to check performance against images or models, a process which itself may involve rapid shifts in mental set. Neither perceptual nor conceptual defects could be adequately accounted for by the Korsakoff patient's memory impairment. Rather, deficiencies in processes common to both might explain the impairment, since experiences insufficiently ordered and integrated are less likely to be available for recall.

SUMMARY

The performance of 24 Korsakoff patients, studied in the chronic phase of the illness by means of a series of experiments, and tested against controls drawn from nonhospitalized alcoholic and from hospitalized neurological patients, has demonstrated three major sources of their deficiency in concept formation. Categories the Korsakoff patients use for classification are looser than those applied by the controls. Korsakoff patients are virtually incapable of discovering the appropriate criteria of categorization if these happen to be different from attributes familiar and therefore first resorted to; they are subject to particularly severe limitations in forming sequential concepts. In addition, they often fail to apply effectively concepts which they have either discovered for themselves or been told, and have grasped and retained in memory. These defects in concept formation are neither explained by the well-known memory impairment in Korsakoff's psychosis, nor entirely by perceptual deficiencies, but appear to be additional to the latter. The defects in perception and concept formation which characterize the Korsakoff syndrome show many parallel characteristics, and suggest dysfunctioning in common basic processes, such as set rigidity, *i.e.* inability to adopt new attitudes of orientation to a situation. The resultant failure to order and integrate experiences could itself be a cause of the more familiar symptoms of memory disturbance.

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HYPNOSIS, SLEEP AND ELECTRO-ENCEPHALOGRAPHY

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For a long time the reality of hypnotic phenomena was questioned, but almost no one nowadays contests their reality and there are constant attempts to establish physiological criteria for them. Studies have been made of the modifications of respiration, the pulse-rate, arterial pressure and reflexes. But all these criteria are circumstantial.

With the advent of the EEG it was thought possible to discover objective criteria of hypnotic phenomena. Research was undertaken and the question was raised as to whether it would be possible to objectify the hypnotic state by means of EEG waves.

As we shall presently observe, these investigations are very difficult and the conclusions contradictory. The principal difficulty is the absence of physiological criteria, qualitative and quantitative, which would allow us to affirm with certainty that the subject is in a state of trance. Without this certainty, we often do not know what we are measuring.

Let us see how the principal investigators in this area, and the present writers, have set about these researches.

The Abbé Faria (1819) was the first to use the term "lucid sleep" to describe the hypnotic state. Since then, terms such as hypnotic sleep, induced sleep, artificial sleep, etc., have been used. The Pavlovian school has given a new formulation to the concept of partial sleep. EEG investigations therefore have had the goal of determining, first of all, whether in the hypnotic state was to be found the electrical activity of physiological sleep.

CRITICAL SURVEY OF THE LITERATURE

In the U.S.A., Loomis, Hobart and Harvey (19) examined a patient under hypnosis in 1936 and found only a minimal difference between the record of the waking state and that of trance. In 1947 Dynes (9) reported on four subjects and confirmed the earlier results, demonstrating that the records of the waking and hypnotic states were identical.

Barker and Burgwin (2) also consider that hypnosis is not a variation of sleep, but their differentiation is more subtle. They make a distinction in hypnosis between a state of hypnotic suggestibility and one of hypnotic sleep. The state of hypnotic suggestibility, characterized by phenomena such as paralysis, catalepsy or anesthesia, does not reveal itself by any modification of the normal EEG record. The mechanism of these phenomena does not involve a bioelectric modification characteristic of sleep. These phenomena can be obtained in a waking state, and they also occur spontaneously in pathological states (*e.g.* hysteria, schizophrenia).

For these authors, hypnotic sleep is a physiological sleep induced under hypnosis, and from the point of view of EEG it is no different from normal sleep. If one stresses induction by appropriate suggestions (and it is not sufficient to say, "Go to sleep"; rather, it is necessary to create an atmosphere of sleep by descriptive suggestions), one obtains—after passing the stage of a flat and rapid record—theta waves characteristic of a light sleep during which the operator remains in communication with the subject. Barker and Burgwin give us few precise details on the form of communication with the subject: it is "a communication necessary to establish effective suggestions." They also succeed in inducing deep sleep (characterized by delta waves) during which any active communication becomes impossible. The hypnotic sleep thus is transformed into natural sleep. "Nevertheless

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these deeper stages could be induced or terminated by appropriate suggestion." This lacks clarity since elsewhere (1) these investigators report that there is a return to the alpha waves at the moment when active communication is suggested. We do not know if the theta rhythm persisted when, for example, the hypnotized subject was carrying out orders.

In a paper published in 1953 the Swiss authors Heimann and Spoerri (14) consider hypnosis to be a phenomenon completely independent of sleep. In their EEG researches they have obtained in all cases a record of the waking state, and this with hypnotized persons who seemed from a clinical point of view to be completely somnambulistic.

Unlike Barker and Burgwin, these investigators were unable to induce sleep by appropriate suggestions. They attempted to create an atmosphere of sleep by descriptive methods, without confining themselves to a suggestion ordering the patient to go to sleep.

Heimann and Spoerri established, with a female patient on whom they experimented throughout one night, that if left alone the subject went off into a light sleep, with corresponding EEG signs. Any noise or light caused the alpha rhythm to reappear without the patient's changing position. The alpha rhythm would disappear when calm returned. In the morning it was necessary to waken the patient by a suggestion which annulled the hypnosis. Despite this light sleep the patient reported a subjective sensation of rest.

These authors postulate the parallel coexistence of two states: sleep and hypnosis. When sleep is deep it can annul the hypnosis and awakening occurs with any indifferent stimulus. When sleep is light, the stimulus does not succeed in breaking the hypnosis; the sleep ceases and the hypnosis persists, and it is necessary to waken the subject by a suggestion.

In 1950, Vesely (29), using a cathode oscillograph, examined three pseudonarcoleptics (narcolepsy with hysterical sleep, lasting one to eight days, in patients who were extremely suggestible). In these subjects, whose records were impaired, the EEG records became normal under hypnosis. It would seem that these were instances of "good" hypnotic subjects, since in all three it was possible to provoke by suggestion the appearance or disappearance of their menstruation. One of these patients underwent a laparotomy under hypnotic anesthesia.

The Russian author Marenina (20), of the Pavlov Institute, reported in 1952 the results of two studies which were also carried out with the aid of a cathode oscillograph. In the first experiment, made on 25 patients in a psychiatric clinic, she studied the EEGs of hypnotic sleep in the first phase, and principally in the second phase characterized by catalepsy.

She found a minimal difference between the EEG of the waking state and that of hypnosis. In the course of hypnosis the alpha rhythm was seen to be less ample and more labile. Accompanying suggestions restored the record to that of a waking state, *i.e.* amplitude was increased. There was a "disinhibition." Therapeutic suggestions reduced the amplitude and regularized the record, indicating a more concentrated relaxation. Marenina never found the record slowing down. In a very prolonged hypnosis, the hypnotic state changed into physiological sleep, with a slowing down of the record. Then the patient could be awakened by any stimulus whatsoever.

In another work (21) Marenina studied the somnambulist phase which she obtained in 25 subjects out of 51 recorded. The somnambulist phase is characterized by the possibility of producing hallucinatory phenomena, by amnesia and by posthypnotic suggestibility. Subjects can open their eyes without emerging from the trance. In this phase an absence of alpha rhythm and a beta rhythm of small amplitude was found.

When the subject opened his eyes the amplitude of the beta rhythm increased. If deep sleep was suggested, the record became flatter; if drowsiness was suggested, the beta rhythm increased in amplitude; the effect was the same after therapeutic suggestions. These modifications were observed in 80 per cent of the subjects studied. In spite of the common clinical tendency to call the somnambulist phase a "deep sleep," Marenina considers that this phase manifests itself neither by a profound nor by a generalized inhibition.

In a more recent study (22), Marenina lays more stress on the relationship between sleep and hypnosis. She again investigates the three hypnotic stages and describes the variations of amplitude and regularity of alpha and beta tracings according to the stages. Especially during the somnambulism stage, the single significant change is a global decrease in amplitude; in spite of the slight differences described between the waking state and hypnosis tracings, she considers this similarity as artificial. During hypnosis, which is a partial sleep, the cortex is inhibited, but cortical foci of excitation persist and create this false likeness. In addition, the sub-cortex retains its activity and also contributes to the existence of this similarity. In Marenina's opinion the other studies failed to investigate sufficiently the various stages of hypnosis.

Krakora (17) follows Pavlov in considering that hypnosis is a partial sleep; a partial inhibition in the topographical and qualitative sense. It affects only certain areas but is incomplete in intensity. Certain cortical points remain "vigilant" as a result of which the subject remains in communication with the operator.

Nevertheless Krakora (here differing from Pavlov) agrees with Biriukov in thinking that hypnosis is confined to the cortex and does not penetrate to the subcortical zone. He quotes Tchougounov who in his treatise on the EEG (28) mentions that the record of hypnosis is identical with that of the waking state. Platonov (24) holds the opposite view.

Krakora has studied the relation between sleep and hypnosis in ten subjects, and has distinguished two states, as follows.

1. A light hypnosis, light inhibition: the "rapport" in which communication exists and where the hypnotized person carries out orders. Here the record is the same as for the waking state, apart from a certain "purification" of the alpha rhythm.

2. At the end of a certain time, after the last of a series of suggestions, the subject, left to rest, passes into a deep inhibition, a hypnotic sleep. The EEG record of sleep then appears.

Subjects always go off to sleep, sometimes at the end of two hours (repetition of the experiment accelerates the arrival of sleep, by the mechanism of the conditioned reflex); but if one addresses remarks to the subject, or if one makes use of an indifferent stimulus, the subject returns to the stage of "rapport." If the sleep lasts several hours and the hypnosis is light, the subject can pass spontaneously into the waking state. But Krakora has never noted a record of sleep when the hypnotized person carried out orders, or when one was addressing a remark to him. During a period of active communication, the record has always been that of waking.

Nevsky (23), following the ideas of Pavlov, considers that hypnosis is a partial sleep, an intermediate state between waking and sleep. He reported the study of 25 subjects, using either the technique of the verbal method or that of fixing the gaze, or a combination of the two methods. No details of the suggestions used were given. Nevsky believes he can distinguish four different hypnotic stages in the EEG.

1. When the eyelids close there is an "equalization phase," indicated by a flattening of the waves.

2. When there is paralysis, the "alpha fusiform" appears.

3. The "stage of minimal activity" then

occurs, corresponding to the passing from moderate hypnosis to deep hypnosis.

4. Finally, there is an appearance of the theta rhythm, corresponding to deep hypnosis or to somnambulism.

Nevsky does not describe clinical criteria for depth of hypnosis. A delta rhythm (deep sleep) was not obtained. According to this writer the change of phases in natural sleep is more rapid than in hypnotic sleep. Nevsky believes that he has experimentally demonstrated that the slowing down begins at the level of the cortex, and afterwards finally reaches the subcortical zones, although technical details are lacking in his report.

Franek and Thren (10), following the theories of Kretschmer, also obtained records of falling asleep with three successive stages.

Giesler (13), reporting on ten hypnotized subjects, found slight changes: less ample and a little slower alpha in the tracing during hypnosis than in the waking state. He interpreted these tracings as a fatigue pattern. Schwartz *et al.* (26) obtained, through sleep suggestions given to seven subjects, three tracings with flattening and four with spindles, V waves and slow waves. It would appear that when communication is established, the alpha waves reappear. Schwartz does not conclude that hypnosis and sleep are similar; in the opinion of his group there is a physiological sleep induced through hypnosis. Israël and Rohmer (15) studied EEG tracings in six subjects during autogenous training. Three of these subjects were also studied during hypnosis. The results obtained with relaxation and hypnosis were similar.[2] These writers distinguish two types of modifications in hypnotized subjects.

1. Decreased amplitude of alpha rhythm, progressive disappearance of alpha, replaced by irregular, slow and low amplitude oscillations of the basic line, with infrequent occurrence of bursts of theta waves.

2. Short paroxysmal bursts; high voltage alpha; complex aspects with intrication of rapid rhythms, alpha and theta waves and spikes; bursts of theta rhythms.

In these authors' opinions the mechanisms implicated in hypnosis would be analogous to those of the period preceding sleep.[3] In effect, the observed changes mainly consist of a flattening and decrease of the alpha rhythm, to be compared with the aspects of a subject falling asleep; the paroxysmal bursts can also be compared to the spindles of the period preceding sleep. However, slow waves are lacking and the spindles are not "true" spindles.

In this review of the literature it is apparent that opinions differ as to the nature of hypnosis: for some it is a phenomenon which is not the same as sleep, for others, it is a partial sleep, for others still, a light sleep. Although the findings of the several investigators appear various, a closer inspection reveals that there are certain similarities.

Most of the authors from 1948 on, have succeeded in obtaining *a tracing of light sleep after starting from hypnosis.* What differ principally, then, are the interpretations made of the phenomena.

Barker and Burgwin consider their tracings to be those of a normal sleep induced under hypnosis, which they call hypnotic sleep. Schwartz *et al.* reach similar conclusions. Heimann and Spoerri see sleep occurring spontaneously; they conceive of a parallel coexistence of these two states. When

[2] There are discrepancies between authors on the relationship between relaxation and hypnosis. Jacobson (16), the author of the technique of progressive relaxation, thinks that relaxation is a phenomenon completely independent of hypnosis, and that hypnosis does not necessarily involve any relaxation. As against this, Schultz, the author of another technique of relaxation (autogenous training), considers that his own method is derived from hypnosis. This is also the case with Kretschmer's "graduated hypnosis." In the course of our recordings, hypnosis subjects sometimes have had abundant fronto-palpebral myograms at the start of the hypnosis, but these diminished progressively.

[3] In French, "présommeil" ("pre-sleep"); light sleep, falling asleep.

Marenina obtains a theta record, she concludes that there is a shift from the hypnotic state to normal sleep. Krakora calls hypnotic sleep a sleep record in a subject who falls asleep starting from hypnosis.

Nevsky speaks of hypnotic sleep as a partial sleep. Israël and Rohmer consider mechanisms related to those of light sleep.

The essential problem, that of knowing whether it is possible to obtain a sleep record while maintaining communication with the subject, is not always clearly pointed out. Barker and Burgwin do not sufficiently clarify this point in their publications.[4]

Heimann and Spoerri, and Krakora, note that the alpha rhythm reappears when communication is established. Marenina is no longer in communication with the subject when the sleep record appears. Nevsky seems to maintain communication, since the subject is in a state of somnambulism, but he gives few exact details on the nature of the communications and we should like to know if, for example, execution of orders by the subject is compatible with a persistence of the theta rhythm.

PERSONAL INVESTIGATION

Our study, made in the EEG laboratory of La Pitié (H. Fischgold) dealt with ten subjects, taken for the most part from the outpatient department of the Villejuif Centre of Psychosomatic Médecine (M. Montassut) and from the outpatient department of Urology at the Hôpital Saint-Louis (Professeur agrege P. Aboulker).

The technique of ocular fixation and of verbal suggestion was used. All of these patients, in hypnotic terms, could be called "good subjects", and some of them were somnambulists in the classic sense of the word. Almost all were conditioned to go to sleep when the experimenter counted up to a certain number. We tried all kinds of suggestions; sometimes without suggesting sleep, but occasionally mentioning it and even stressing it by means of descriptive suggestions.

At first a six-channel apparatus was used; then (beginning with the sixth subject) a Reega with 12 channels. The recording was always bipolar.

Subject 1; a female, aged 30. Waking: alpha occipital at 9 c/sec., regular and symmetrical, fairly rapid, interspersed with rare patches of rapid rhythms (Figure 1A). After hypnosis: immediately after closing of eyelids, alpha absolutely identical and persisting thus throughout two hours of recording. No modifications, even when the patient was left alone in the dark for a long time, or when descriptive suggestions were given (Figure 1B).

Subject 2; a female, aged 22. Record clearly asymmetrical; alpha at 9 c/sec., with much greater amplitude on the right. The hypnosis, from beginning to end of the experiment, did not modify the alpha rhythm which remained asymmetrical. No signs of flattening out or slowing down, in spite of suggestion to sleep.

Subject 3; a male, aged 20. Before hypnosis: alpha rapid at 12 c/sec., symmetrical (Figure 2A). Under hypnosis: ocular revulsion at number 7; closing of eyelids (no suggestion as to sleep), followed by the appearance of alpha rhythm, then at once a flat record with numerous rapid rhythms. At the end of 30 seconds the record appears as follows: small alpha patches of lesser amplitude, interspersed with flattening-outs for a few seconds and infrequent isolated rhythms at 6–7 c/sec. Disappearance of palpebral artifacts.

Three minutes later the patient carries out a suggestion (raises the right arm); the record remains flattened and slowed down; infrequent alpha and fairly abundant theta (Figure 2B). This aspect seems to persist.

[4] In a personal communication, Barker states that she never obtained a sleep record of any lasting kind when she was in communication with the subject. It sometimes happens that the subject might obey a suggestion while still maintaining the record of light sleep, but this is exceptional, and there is always a momentary return of the alpha rhythm.

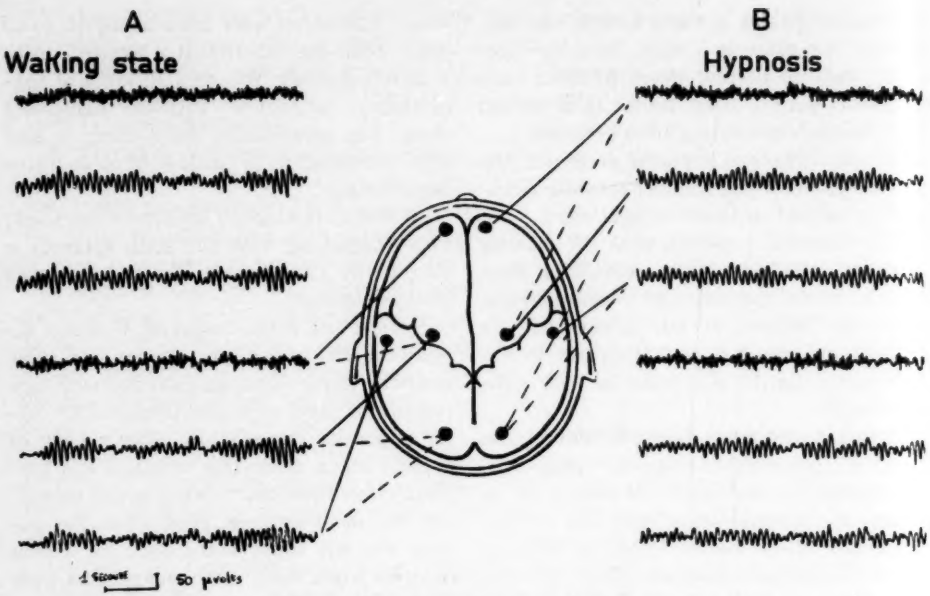

FIG. 1. EEG record of Subject No. 1, discussed in text. A: before hypnosis, alpha at 9 c/sec. B: after ten minutes of hypnosis; alpha unchanged.

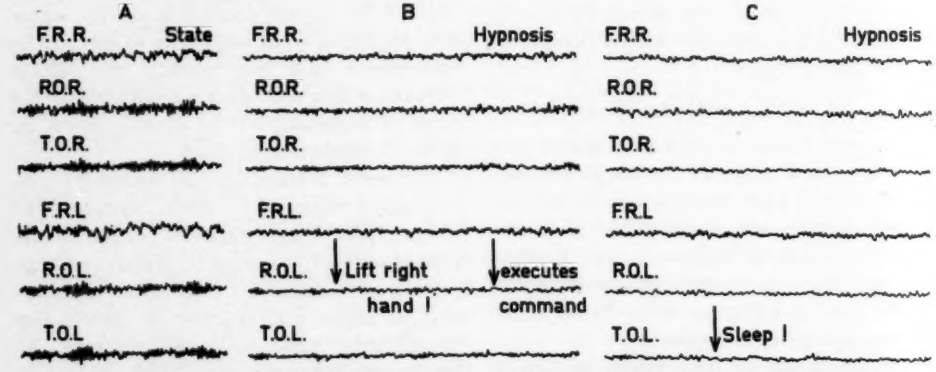

FIG. 2. EEG record of Subject No. 3, discussed in text. A: before hypnosis; alpha rapid at 12 c/sec. B: under hypnosis. At the suggestion, "Lift the right hand," the patient carries out the order. The record remains slow and flattened, with rare alpha and fairly abundant theta. Note disappearance of palpebral artifacts. C: after four minutes of hypnosis. The record is rich in theta. Sleep is suggested to the subject; the record is unmodified.

The patient is spoken to without his waking up and without any return to the alpha rhythm (Figure 2C). The third hypnotization produces modification of the record much more quickly than the previous ones, and a flattening of the waves is observed. In addition, there are abundant theta rhythms, less than two minutes after the beginning of hypnosis. Variability of the reactions to external noises or to the operator's

remarks is noted. A brief reappearance of the alpha is just as likely to be observed as is an unmodified record.

Subject 4; a male, aged 25. Waking: occipital alpha at 10 c/sec., symmetrical and ample. Under hypnosis: during induction, at the beginning there is no modification of the record; suggestion of sleep was not used. After about ten minutes, ideas of sleep were suggested, without modification of the record. The patient was then left alone: after five minutes, there were periods of brief flattening out of the record for about ten seconds, sometimes accompanied by infrequent isolated theta rhythms. At the slightest external stimulus, a patch of alpha of 10–20 seconds duration occurred, even at the suggestion, "Go to sleep" (Figure 3). With the return of silence the record once again becomes flat and the theta rhythm becomes more frequent.

Subject 5; a female, aged 56. In a waking state her record is regular. Alpha spontaneous and rapid, at 11 c/sec., with a distinct return after closing of the eyelids; inter-

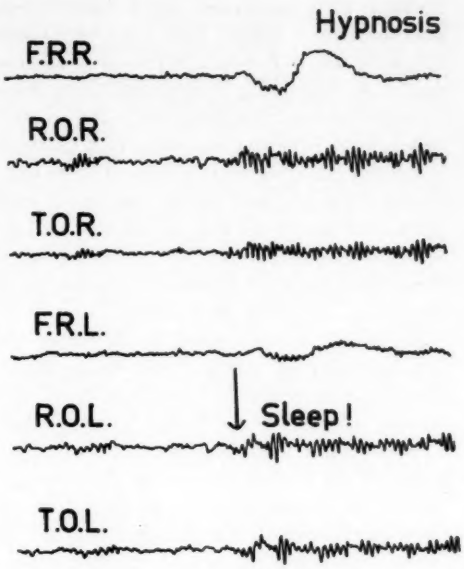

FIG. 3. EEG record of Subject No. 4, discussed in text. Under hypnosis with the patient undisturbed, the record is flat, with some theta rhythms. At the suggestion to sleep, the alpha reappears as at the beginning of the record.

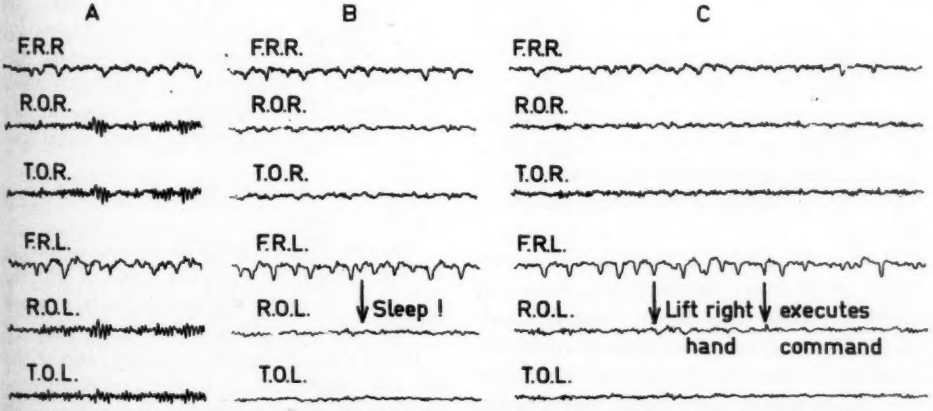

FIG. 4. EEG record of Subject No. 6, discussed in text. A: waking state. Alpha irregular at 10–11 c/sec., with movements of eyelids. B: after fifteen minutes of hypnosis the record is flat and irregular, unmodified by suggestion of sleep. Irregular movements of eyelids. C: under hypnosis, three minutes after point B. Record is flat and irregular, but less than at point B. Although the subject responds to the suggestion to raise his hand, the record is unmodified.

spersed with numerous rapid frequencies, and sometimes loaded with very rare theta elements at 6–7 c/sec. without predominance. At beginning of hypnosis there were no appreciable modifications; the record was always irregular and very rich in rapid rhythms; the same state persisted after suggested catalepsy and other tests. At a suggestion of nocturnal sleep there was no immediate change. After ten minutes, some theta rhythms were observed, slightly more abundant and very transitory.

Subject 6; a male, aged 31. Waking: alpha irregular at 10–11 c/sec. Some rapid rhythms and numerous movements of the eyelids (Figure 4A). Hypnosis introduced no appreciable modification, and the movements of the eyelids continued. At the suggestion of nocturnal sleep, an imitation of sleep was observed, without modification of the record. Ten minutes later, during which time the operator was present but silent, the record flattened out. The alpha decreased in amplitude and then disappeared; the tracing flattened out, became richer in rapid rhythms whereas oculo-palpebral artifacts persisted. During the suggestion, "Go to sleep," the record remained flat and irregular (Figure 4B). The subject was twice ordered to raise his right hand; he did not move and the record was unmodified. The third time the patient did make the suggested movement, but the record remained flat, without any well-expressed alpha (Figure 4C), and then resumed its former aspect. After rehypnotization, flattening out is not obtained.

Subject 7; a female, aged 34. Alpha occipital at 10 c/sec., irregular, interrupted by rapid rhythms. Numerous muscular artifacts. In the course of different hypnoses, undertaken successively, the record retained the same irregular aspect; the artifacts being slightly less numerous. There was no slowing down, and no significant flattening in the course of the recording. A very strong sensory stimulus (ammonia) caused the subject to emerge from the hypnosis, without the record being modified.

Subject 8; a female, aged 20. Alpha at 9 c/sec., better expressed on the right, interrupted with some oscillations of 5–7 c/sec.; completely removed on opening the eyelids. No modification occurred during hypnosis. It is interesting to note that the patient was accustomed to going into a deep trance. The same impression was formed during the experiment, but the subject declared after waking up that she had not been in a trance, and that the head-piece had disturbed her.

Subject 9; a female, aged 22. Spontaneously: alpha occipital at 11 c/sec., low voltage, in short bursts interspersed with periods of irregular flattening. The same aspect was observed under hypnosis. No single modification of the record took place, even when sleep was suggested.

Subject 10; a male, aged 25. Waking: alpha at 9–10 c/sec., rather irregular spontaneously. Hypnosis: after closing of the eyelids, alpha even less regular and less ample, and requiring a minute to regain its former aspect. A second hypnosis made it possible to pass more quickly to the alpha of the waking state. Suggestion of sleep: the patient was left alone for several minutes, and flattening of the record was noted for brief periods (ten seconds), with the appearance of a theta at 5–7 c/sec. of small amplitude. The reactivity of the subject could not be studied during these short periods.

It is entirely clear that these results are highly variable.

The two first subjects showed no appreciable difference between the state of waking and that of hypnosis, apart from a more distinct alpha rhythm in the latter state.

In the case of the third subject, and without any suggestion as to sleep, a *theta record was obtained, while communication with him was maintained.* This patient carried out orders without any change in the record. External stimulation merely modified the record in an intermittent manner.

With the fourth subject, on the other hand, in spite of suggestions of sleep, a theta record was not immediately obtained. It occurred if the patient was left alone, while external stimulation caused the alpha to reappear.

With the sixth subject, the alpha rhythm disappeared; the tracing was flat and rich in rapid rhythms, and oculo-palpebral artifacts persisted. Communication with the patient was maintained and he carried out orders with no modification of the record.

The seventh and ninth subjects showed no modification of the record after hypnosis.

With the eighth subject it was noted that her posture (which is that of one hypnotized) did not correspond to that of a deep trance.

The tenth subject demonstrated momentary evidence of a sleep record, but these were insufficient for communication with this patient.

DISCUSSION

Clearly, what characterizes the different studies reported is their disparity. It is difficult to account for this variation without a close examination of the nature of the hypnotic trance. Psycho-physiological aspects of the hypnotic state are, however, still little known. In this discussion we shall try to develop some ideas to throw at least some light on the apparently contradictory results of different investigators.

Writers in this area speak of a hypnotic state as a uniform state, and of *the depth of the trance* as a vertical dimension. Even the early investigators, while denoting a trance as light, moderate or deep, emphasized that there were no constant phenomena characterizing the depth of the trance. For example, the anesthesia characteristic of a medium trance could occur in a light trance, and even in a waking state. The same is true of amnesia and other phenomena. Bernheim remarked that every sleeper has his own individual characteristics.

The trance is mobile and fugitive: it is subject to continual fluctuations; it can undergo modification in the course of one hypnotic session, and from one session to another. It has not been proved that hypnosis becomes deeper with "training," although Forel maintained an opposite opinion. The present writers believe that this is an entirely individual function. For example, with the fifth and ninth patients of our series, the trance was much deeper at the beginning than at the end of the treatment.

The depth of the trance can also vary—as has been shown by Brenman, Gill and Knight (3)—according to the degree of anxiety which is produced by the hypnotherapeutic situation, or by the material discussed in the course of the sessions, or as a function of the structure of the ego. Together with Melle Cahen (6) we were able to establish this on a number of occasions, in the course of treatment and notably with the sixth subject of this series, who awoke from a deep trance when he began to recall a traumatic experience.

Certain authors believe that the contrary phenomenon is equally possible. That is to say, the sliding from hypnosis to a deep natural sleep can sometimes constitute a mode of defense.

The psychological and the physiological participation of the subject varies considerably with respect to intensity. When the hypnosis takes place more on the psychological level, it is the phenomenon of "role playing" that is predominant. Sometimes, on the other hand, the mobilization of the physiological reactivity plays a more important role. We know of metabolic and other modifications which can be produced under hypnosis.

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As regards the connection with sleep, the problem is complex. Numerous writers agree that there is a certain resemblance in psychological functioning during hypnosis and during sleep. From the physiological point of view (apart from EEG) the objective criteria are circumstantial and—above all—lacking in constancy.

EEG researches do not provide irrefutable proofs, confirming or invalidating the hypothesis as to the similarity between sleep and hypnosis, from the point of view of bio-electrical activity.

Nevsky's position in favor of *similarity* is not conclusive, for he does not state clearly what his criteria for somnambulism are. One wonders whether he is thinking of the classic criteria, according to which the subject opens his eyes, speaks, moves about, without emerging from the trance. Or again, does he believe there is somnambulism when the sleep record appears? It would therefore be interesting to learn if Nevsky's 25 subjects were chosen at random, or were already known as "good subjects," or indeed were chosen from a group which was more significant because it already showed this record.

Krakora's position is even more questionable, for it could be critically maintained that he cannot prove that what he calls the "rapport" (corresponding to a waking record) can be shown by physiological criteria to be a hypnotic trance.

It is difficult to support the sleep theory by means of the very slight modifications discovered by Marenina.

The opinion of Israël and Rohmer is more subtle and the analogy with light sleep is discussed. In their reports the problem of communication is not considered.

From the point of view of proving *non-similarity*, the observations of Barker and Burgwin are unsatisfactory. They obtain no record of sleep during the carrying out of somatic suggestions, such as paralysis or catalepsy, since (according to them) these phenomena can be obtained in a waking state. Accordingly it might be maintained that their subjects are not in a trance, or else in a very light trance, which does not produce any modification of the record.

We do not feel that the slight distinction introduced by descriptive suggestions as to sleep is meaningful. In some cases we have obtained theta waves (light sleep) without any suggestions as to sleep.

Gorton (12), commenting on the studies of Barker and Burgwin, considers that in a case where a sleep record is obtained, it is a matter of a communication existing between the operator and the subject sleeping naturally. It could be retorted that when one is in communication with a sleeping person, the latter must be in a state of spontaneous hypnosis.

In our studies most tracings failed to display any significant differences between the waking state and hypnosis. However, one might discuss the tracings of four of our subjects in whom a marked difference appeared during hypnosis. In two subjects (Nos. 4 and 10) light sleep tracings were seen, but any external stimulation caused the waking alpha rhythm to reappear.

The third subject presented aspects close to those of falling asleep, even during communication.

Our sixth subject presented aspects which may be interpreted as bearing evidence of an increased attention (arousal)[5]: oculopalpebral artifacts, flattened tracing, irregular and rapid in the posterior areas. Nevertheless, the intrication of fairly rare theta waves raises the problem of light sleep. Besides, one might wonder whether this contradiction is only apparent and whether certainly falling-asleep states would not be accompanied by an effort of mental concentration. This should be related to the EEG pattern of the "yoga" trance described by Das and Gastaut (8). These investigators found that middle trance conditions are chiefly evidenced by a slow activity, whereas ecstasy is accompanied by very ample rapid rhythms.

Darrow, Henry, Gill and Brenman have

[5] Following the diencephalon and the brain cortex, the center of hypnosis will certainly tentatively be located in the reticular formation. This has already been suggested for animal hypnosis by Svorad; however, he distinguishes animal hypnosis from human hypnosis: the former being located in the sub-cortex, the latter in the cortex.

begun to approach this problem from a different point of view, by studying the relation in bio-electric activity between the different cerebral zones. This investigation appears still to be in a pre-publication stage.

CONCLUSION

Electro-encephalographic investigations, in the present state of this technique, cannot furnish irrefutable proof regarding the question of similarity or dissimilarity between the hypnotic state and sleep. In several of our subjects, hypnosis failed to modify the tracing; in others, we obtained light sleep patterns, but the alpha rhythm reappeared as soon as the communication with the outer world was established. Other records yielded tracings the interpretation of which could not be unequivocal, changing states of consciousness were suggested. Slowing down of the tracing (hypoactivity) suggesting a light sleep condition; dyssynchronization of the alpha activity with accentuation of ocular artifacts suggesting a hyper vigilant state (hyperactivity). Execution of orders did not modify the tracing.

The number of subjects studied is not sufficiently large to provide definitive conclusions. Sometimes the hypnotic state manifests itself largely on a psychological level; at other times the manifestation is largely physiological.

As long as hypnosis lacks objective physiological criteria, both qualitative and quantitative, the results of research in this field will remain inconclusive.

Nonetheless these investigations are of considerable interest and should be experimentally pursued with larger numbers of subjects. Such studies might well yield important psychophysiological contributions.

SUMMARY

This is a critical survey of studies dealing with hypnosis and EEG which have appeared in the U.S.A., the U.S.S.R., Czechoslovakia, Switzerland and Germany. It raises the problem of the similarity or dissimilarity between hypnosis and sleep, from the electro-encephalographic point of view. Discrepancies, noted in the conclusions of the writers in this field, seem to be due primarily to methodological difficulties and to the complexity of the hypnotic phenomenon, the physiological basis of which is fluctuating and inconstant.

The authors report a study of ten subjects whose records of the waking and hypnotic states were examined. The results obtained are varied: in six subjects, there was no significant difference between the waking state and hypnosis tracings; in two subjects, tracings of light sleep were obtained, but the waking state pattern reappeared during active communication with the outer world. In two subjects, tracings difficult of interpretation were found; in one of the latter, the slowing down of the tracing might suggest a slight sleep condition. In the other the dyssynchronization of alpha activity and accentuation of ocular artifacts might suggest a hyper vigilant state. In both cases active communication did not modify the record.

The inconstant nature of the objective criteria of hypnosis, both qualitative and quantitative, make it difficult to carry out research and to draw definitive conclusions. Nevertheless hypnosis has interesting experimental possibilities, and is a potential field for psychophysiological research.

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THE EFFECT OF ELECTROCONVULSIVE TREATMENT UPON THE PERCEPTION OF THE SPIRAL AFTEREFFECT, A PRESUMED MEASURE OF CEREBRAL DYSFUNCTION

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In the quest for simple tests to measure the extent and nature of brain damage, several investigators have recently focused their attention upon the spiral aftereffect. In 1955 Price and Deabler (4) reported that individuals with brain damage could be differentiated from the non-brain-damaged with a high degree of certainty. Stilson *et al.* (6) in reviewing the statistics of previous workers (3, 4) concluded that the test could only be used with confidence to differentiate "nonorganic" from "organic" patients. Although Gallese (1) presented confirming evidence, he observed that the test was relatively insensitive to the presence of epilepsy, to chronic brain syndrome associated with alcoholism, and to lobotomy in schizophrenics.

Thus far the test has seldom been used in the investigation of patients receiving electroconvulsive treatment. Standlee (5) examined patients eight hours after an electroconvulsive treatment and found no change in 23 of 25 cases. Goldberg and Smith (2) worked with patients one to seven days after a course of four or more treatments and found little relationship between perception of the aftereffect and "organicity."

The purpose of this investigation was to determine whether there was any relation between the "organic mental changes" noted after electroconvulsive treatment and ability to perceive the spiral aftereffect. If such a relationship were found, the test could prove useful as a measure of "organicity" in evaluating the effects of shock therapy.

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METHOD

Apparatus. Two Archimedes spirals of two and one-half turns (920°), 10 inches in diameter and mirror images of one another, were used (Figure 1). They were rotated clockwise on a phonograph turntable at 78 rpm, for thirty seconds, after which movement was manually and instantaneously stopped. The normal response to rotation of Spiral A is to describe it as contracting and receding from the observer. When rotation is stopped, a negative afterimage usually ensues. This appears to increase in size and move toward the observer. Spiral B has the reverse effect.

All patients were seated eight feet from the turntable, which was perpendicular to the floor and at eye level. The spirals were presented four to six times in random alternating order.

Instructions were similar to those of Price and Deabler (4). The patient was asked to look at the center of the spiral and to describe what he saw. This was done to ensure his attending to the stimulus. When the rotation was stopped, each patient was asked, "Now what do you see?" If he did not see the negative afterimage, he was carefully questioned as to the nature of his perception.

Patients. All of the 31 patients referred for electroconvulsive treatment during the course of the experiment were tested. Four patients never perceived the aftereffect at

FIG. 1. Archimedes Spiral A. Spiral B is mirror image of A.

any time during the experiment and their data were excluded from statistical analysis. Patients were treated three times a week, usually for a course of 10 to 20 treatments. It was hoped that testing could be done with every treatment, but this did not prove feasible. Instead, the patients were tested as often as possible, the majority on at least six different days. Some were tested on as many as 12 days.

Patients were tested before treatment, and again 45 to 60 minutes after treatment. This time was chosen as the earliest for achieving satisfactory contact.

In the routine pretesting interview, attempts were made to establish the degree of improvement, and the presence of an organic mental syndrome.

Controls. Two sets of controls were used: 80 normal controls, who were professional and nonprofessional members of the hospital staff; and eleven psychotic patient controls, who were similar to the experimental group but had not yet begun shock treatment. The patient controls were tested on five different days.

Despite the fact that previous reports have indicated that nearly all individuals without impairment to the brain perceive the spiral aftereffect, it was decided to verify this by testing two separate groups. Ten normal controls were examined individually, and approximately 70 were tested in a group. In no case did a normal individual fail to perceive the spiral aftereffect. The patient controls perceived fewer aftereffects than did the normal group and showed a slight increase in their level of perception when the test was repeated one hour later (Figure 2). However, many of the patients were inconsistent in their responses from trial to trial.

In order to evaluate the effect of electroconvulsive treatment on the spiral aftereffect, a two-factor analysis of variance was performed in which the pre- and postshock conditions were compared for the first half of the course of treatments and for the second half. It can be seen that there was a significant decrease in ability to see aftereffects in the immediate postshock period. However, no significant difference in score was found when the frequency of perception of the aftereffects for the first half of the tests was compared with the frequency for the second (Table 1).

There was a significant interaction between the variables of shock condition and the first and second half of the series. Table 2 shows that there was a significant decline in perception of aftereffect from the pre- to the postshock sessions for the series as a whole, as well as for the first and second halves of the course of treatments. However,

FIG. 2. Percentage of Perception of Spiral Aftereffect.

the decline in perception of the aftereffect is more pronounced in the second half, which accounts for the significant interaction.

A comparison was made between the schizophrenic and depressed patients to determine whether there was a differential effect in the ability of these groups to perceive the aftereffect. All patients were placed in one of two categories: those who showed a decrease in aftereffect from the pre- to the postshock period, and those who did not. Nine of the 15 depressed patients and six of the 12 schizophrenic patients showed a decline in score. The chi-square test was nonsignificant, indicating that the spiral aftereffect cannot be utilized diagnostically.

In spite of the fact that there was a significant decline in the performance of the total group after shock, it must be pointed out that only 15 of the 27 patients showed a decline in score after shock. The remaining 12 either showed no change after electroconvulsive treatment or a slight increase in the number of aftereffects they perceived. This fact is reflected in the analysis of variance (Table 1) by the significant interaction between Subjects and Pre- Post Shock. Apparently the reason the entire group showed a highly significant decline in this measure after electroconvulsive treatment was because many of the patients had a considerably poorer score, while those who improved did so only slightly.

TABLE 1

Summary of Analysis of Variance for Percentage of Spiral Aftereffects Before and After ECT

Source of Variance	SS	df	MS	F
Between Subjects (Ss)	91,021.80	26	3,500.84	
Pre-Post Shock (PP)	7,633.93	1	7,633.93	10.52***
First-Second Half (FS)	334.27	1	334.27	<1
PP x FS	725.91	1	725.91	1.97*
PP x Ss	24,973.57	26	959.91	2.60**
FS x Ss	13,911.23	26	535.05	1.45
PP x FS x Ss	9,594.59	26	369.02	
Total	148,195.30	107		

* $p < .05$
** $p < .01$
*** $p < .001$

TABLE 2

Test of Significance (t) Between Means of Pre- and Postshock Conditions and Between First and Second Halves of Tests

	t	*p*
Preshock vs. Postshock		
First half	4.20	<.001
Second half	2.22	<.05
First half vs. Second half		
Preshock	.32	>.05
Postshock	1.66	>.05

DISCUSSION

The hypothesis that electroconvulsive treatment would cause decreased perception of the spiral aftereffect was confirmed, since there was a significant decline in the frequency of aftereffects immediately after shock. However, the hypothesis that prolonged treatments would cause a decline in score was not confirmed. In fact, there was practically no difference in the frequency of perceived aftereffects in the preshock testing from the first to the second half of the series of treatments (Figure 2). Thus, although electroconvulsive treatment caused an immediate drop in the percentage of perception of aftereffects in the postshock condition, there was no cumulative decline as treatment progressed.

While the pretest interviews demonstrated an increasing degree of "organicity," no relationship was found between such clinical evidence and diminished perceptual ability.

It was our impression that performance on any one trial was not predictive of performance on the next for both the shock and nonshock groups. At times, patients who could not perceive the aftereffect in a given preshock test could do so in the immediate postshock period. There was also no way of predicting from the pretest interview how the patient would perform. Another study,

concerned with the consistency of perception of the aftereffect, is in progress.

A few patients who failed to perceive the aftereffect as the course of treatments began, showed increased perceptual ability as they improved clinically, but this was not true for the majority of patients.

CONCLUSION AND SUMMARY

All of 80 normal individuals who were tested with the Archimedes spiral for perception of aftereffect had a perfect score initially and upon retest. Eleven psychotic "nonorganic" patients who were referred for electroconvulsive therapy were tested on five different days before treatment was initiated. They perceived the aftereffect approximately 60 per cent of the time initially and 65 per cent upon retest one hour later. In 27 psychotic patients who were treated with electroconvulsive therapy, this average tended to decline sharply and significantly in the immediate postshock period. However, there was no continuous decline in score with increased number of treatments. Furthermore, half the treated patients did not show a decline in score immediately after shock, and some never perceived the afterimage. These results indicate that although electroconvulsive treatment decreases the frequency of perception of the aftereffect in some cases, this test is not clinically useful in this population.

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PSYCHOLOGICAL FACTORS AFFECTING INDIVIDUAL DIFFERENCES IN BEHAVIORAL RESPONSE TO CONVULSIVE THERAPY[1]

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INTRODUCTION

While convulsive therapy is generally considered specific for the symptomatic relief of depression and agitation, and for the relief of such "illnesses" as manic-depressive and involutional psychotic reactions, the behavioral response to such therapy is highly variable. In initial attempts at understanding this behavioral variability, differences in physiologic response were sought. Neurophysiologic change was measured in various ways (4). The quantitative measures of induced EEG delta activity (1) and changes in language after amobarbital (3, 7) provided the best indices. Considerable variability in these indices among patients with equivalent numbers of treatment was observed. We concluded that the development of an alteration in brain function, as measured by a high degree of EEG delta activity (1) and positive amobarbital tests (7) was a prerequisite to behavioral change in convulsive therapy. It was apparent, however, that such changes, although necessary, were not sufficient for improvement (2). Indeed, among patients with maximal neurophysiologic change, all patterns of behavioral adaptation were manifest, and ratings of improvement ranged from "recovered" to "unimproved" and "worse."

Equating segments of the observed population according to nosologic or symptomatic categories also failed to explain the variability in behavioral response. While among patients in the manic-depressive and involutional depressive groups a higher incidence of hypomanic and euphoric modes of adaptation were observed, and thus ratings of "recovered" and "much improved" were more frequent, there still were many subjects in these groups who manifested paranoid and somatization modes, and were rated "unimproved."

In the investigations of convulsive therapy, various tests of perceptual organization and indices of sociologic background have been studied which reflect the individual differences in the subjects. Of these, some measures correlated highly with the behavioral response to convulsive therapy. The psychological measures employed have been Rorschach responses (11), "explicit verbal denial" tendencies as measured in structured interviews with family members (12), and scores on the California F Scale (8, 10). The sociologic variables have been chronologic age, years of education and place of birth. It is the purpose of this report to summarize the observations of the relationship between these indices and the variability of the behavioral response to convulsive therapy as reflected in evaluations of improvement.

METHODS

The population has been consecutive referrals for convulsive therapy in a voluntary, non-profit, urban psychiatric hospital. Patients were generally Jewish, of low and middle socio-economic classes with a mean educational level of 10.5 years. Ages ranged from 16 to 67 with a mean of 41 years. Diagnoses included schizophrenia, manic-depressive, psychoneurotic and involutional depressive reactions. As segments of the population were studied by various procedures at different times, the tables reflect

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the different numbers of subjects that were included in each procedure.

All patients received electroconvulsive therapy three times a week, using either unidirectional or alternating current instruments. The various psychological tests were administered within the week prior to treatment.

We have previously described the behavioral changes in convulsive therapy as variations of five modes of adaptation (euphoric, hypomanic, somatization, paranoid withdrawal and panic), and emphasized that the evaluations of "improvement" in convulsive therapy are value judgments of the induced behavioral changes (2). Patients who manifest euphoric and hypomanic adaptive modes are those generally rated as "much improved" and "recovered" by therapists and administrator, while those who manifest paranoid-withdrawal, somatization or panic modes are generally regarded as "unimproved" or "worse." For this report, evaluations of the patient's behavior and ratings of improvement were made either two to three weeks after termination of treatment (Tables 1, 2, 3) or at the time of discharge from the hospital (Table 4).

TABLE 1

Relation of Rorschach Factors to Clinical Response in Convulsive Therapy

	N	Much Improved	Moderately Improved and Unimproved
Human Movement (M)	39	11 (28%)	28 (72%)
No Human Movement	48	28 (58%)	20 (42%)
	$\chi^2 = 6.76^*$ $p < .01$		
Form Color (FC)	34	7 (21%)	27 (79%)
No Form Color	53	32 (60%)	21 (40%)
	$\chi^2 = 11.57^*$ $p < .001$		
Both M and FC	24	4 (17%)	20 (83%)
Either M or FC	25	10 (40%)	15 (60%)
Neither M nor FC	38	25 (66%)	13 (34%)
	$\chi^2 = 14.83$ $p < .001$		

* With Yates' correction for discontinuity

OBSERVATIONS

RORSCHACH TEST PATTERNS

The Rorschach tests were administered in conventional fashion and scored according to established criteria (13) for specific factors as number and type of movement, color, form, shading and total number of whole responses. For each of these factors, significant differences were observed between the group of patients rated as "much improved" and the combined groups of those patients evaluated as "moderately improved" and "unimproved." Subjects with human movement responses were evaluated as "much improved" significantly less often than subjects without such responses. The presence of form color responses was significantly correlated with lack of improvement, and when this factor was combined with human movement, the ratings were significantly poorer than when neither form color nor human movement were reported (Table 1). Similarly, patients rated "much improved" gave fewer total responses, fewer total movement responses and fewer content categories; but the per cent whole, popular and form responses were significantly greater than in the groups rated as "unimproved" and "moderately improved" (Table 2).

"DENIAL PERSONALITY" SCORE

In their study of denial of illness, Weinstein and Kahn (14, 16) described the characteristics of an "explicit verbal denial" personality type.[3] In an initial group of convulsive therapy patients, the hypothesis was tested that those patients who most closely approximated this personality type would be most likely to be rated as "much improved." "Denial personality" scores were

[3] "They were people with compulsive drives, a great need for prestige and the esteem of others, and a record of always having denied felt inadequacies. ... Life experiences had been valued not for their intrinsic satisfactions but as a means of maintaining prestige and "security." (14).

established pretreatment in independent structured family interviews. Fifteen specific areas of behavior were assessed and scores of 0, 1, and 2 were assigned for each of these areas according to whether the subject least, moderately or most approximated the characteristics of the "explicit verbal denial" personality type. In interviews with relatives of 47 patients, scores ranged from zero to twenty-five, with a median of eleven. Subjects with scores above eleven were classed into a high denial group, while those with scores below, into a low denial group.

Patients with high denial personality scores were most likely to be rated as "much improved," with only one patient rated as "unimproved." Of patients with low denial scores, clinical ratings occurred on a chance basis in each evaluation category (Table 3). The difference in the denial scores between the much and moderately improved patients, when compared to the unimproved patients, is statistically significant at the one per cent level (12).

In a further elaboration of these personality types, studies of the total in-patient population were undertaken. Certain sociologic and psychological factors were studied in all patients in residence on March 7, 1957. These included the California F Scale, age, years of education and place of birth (8).

CALIFORNIA F SCALE

The California F Scale consists of a series of extreme or stereotyped statements concerning social and personal attitudes. The subject reads each statement and then reports the extent of his agreement or disagreement. Originally presented as a guide to a subject's capacity for ethnocentrism and authoritarianism, the method has recently been explored as a measure of stereotypy and rigidity in communication (8, 10). The test was presented to all patients prior to treatment and scored on a scale of ten to seventy. The figures represent maximal disagreement (low score) and maximal agreement (high score) with the statements.

TABLE 2

Relation of Rorschach Factors to Clinical Response in Convulsive Therapy

	N	Mean	S. D.	Difference	t
Number of Responses					
Much Improved	38	13.00	6.7	6.5	2.7**
Moderate, Unimproved	48	19.5	12.8		
Per Cent Whole Responses					
Much Improved	38	37.6	21.0	13.2	3.0**
Moderate, Unimproved	48	24.4	18.2		
Per Cent Popular Responses					
Much Improved	38	37.7	21.6	11.1	2.8**
Moderate, Unimproved	48	26.6	14.3		
Number Movement Responses					
Much Improved	38	2.3	2.7	2.6	2.7**
Moderate, Unimproved	48	4.9	5.1		
Number Content Categories					
Much Improved	38	3.8	2.2	1.1	2.1*
Moderate, Unimproved	48	4.9	2.3		
Per Cent Form Responses					
Much Improved	38	71.8	19.0	9.9	2.2*
Moderate, Unimproved	48	61.9	21.4		

** Significant at .01 level
* Significant at .05 level

TABLE 3

Relation of Denial Personality Scores to Clinical Response in Electroshock

	N	Much Improved	Moderately Improved	Improved
Personality Score				
High Denial—(11–25)	24	14	9	1
Low Denial—(0–10)	23	7	9	7

There was a significant relationship ($p < .05$) between the pretreatment test scores and evaluations of the clinical response to convulsive therapy (Table 4). For patients rated as "recovered," the mean F score was 53.1, while for those rated as "unimproved"

TABLE 4

Relation of Social Factors to Discharge Ratings in Convulsive Therapy

	N	Mean F Score	Mean Age	Mean Years Education	% Foreign Born
Recovered	8	53.1	51.6	9.4	50
Much Improved	26	41.8	43.8	10.6	35
Improved and Unimproved	23	39.7	32.3	12.3	17

the score was 39.7, reflecting greater degrees of agreement with the stereotyped statements of the test for the "recovered" group.

SOCIOLOGIC FACTORS

When analyses were made of the relation between improvement ratings in convulsive therapy and age, years of education and place of birth, significant relationships were observed for each of these variables. The "recovered" patients were significantly older ($p < .001$) and had significantly fewer years of schooling ($p < .05$) than the "unimproved" group. While a larger percentage of the "recovered" patients than the "unimproved" patients was foreign-born (50 per cent *vs.* 17 per cent), the differences were not significant. In each category, the "much improved" subjects fell in between (Table 4).

DISCUSSION

We have noted that measures of perceptual organization, personality traits and sociologic data are related to the degree of improvement shown by subjects with convulsive therapy. These observations provide an understanding of the individual variability in the behavioral response to convulsive therapy under conditions of apparently equivalent degrees of altered brain function.

In their studies of patients with brain disease, Weinstein and Kahn described behavioral patterns as ludic behavior (15), increased smiling and laughter, denial of illness, minimization and displacement of symptoms, and altered sexual behavior achieving prominence in the milieu of altered brain function. They suggested that the manifestation of these behavioral patterns also provided the basis for the evaluation of improvement in convulsive therapy (16). In these studies of patients in convulsive therapy the same patterns of laughing and smiling, denial, displacement, minimization and altered sexual activity do indeed occur in the milieu of the induced altered orientation and discrimination (2). It is the patients demonstrating these altered behavior patterns who are rated as "recovered" or "much improved"; while those patients failing to show these patterns or doing so transiently are evaluated as "unimproved" or "improved."

Recent studies of changes in language with convulsive therapy have further amplified an understanding of these behavioral responses. Alteration in syntactic aspects of language has been related to clinical ratings (9). Patients evaluated as "recovered" and "much improved" demonstrated significantly greater use of the past or future tense and the third person mode with qualification, evasion, denial, displacement, clichés, and cryptic and stereotyped expressions during treatment than did "unimproved" patients. More recently, Jaffe *et al.* (6) reported that formal speech patterns also were characteristically altered. In dyadic interactive speech analyses (5), increased repetitiveness and stereotypy were associated with syntactic language changes during convulsive therapy.

In the studies reported here, aspects of personality organization have been defined which are related to the type of behavioral response incident to convulsive therapy. The Rorschach patterns of the more favorably rated group are generally associated with greater degrees of conventionality and stereotypy, and little introspectiveness, imagination, empathy and creativity. Similarly, the higher F scores of the more favorably rated group is consistent with greater

degrees of ethnocentrism, authoritarianism, rigidity and conventionality. In present day urban culture older patients generally have less formal education and a greater number are foreign born than younger patients. These sociologic factors are also associated with greater adherence to conformist ideologies and ethnocentric identification. We may conclude that those patients who approximate the "explicit verbal denial" personality type, and who are non-empathic, non-introspective, stereotyped, rigid and conventional are most likely to manifest the euphoric and hypomanic modes of behavior under the conditions of altered brain function induced by repeated convulsions. Such patients also rely primarily on nonverbal patterns of communication, and with treatment evince increasing use of the language patterns of repetitiveness, denial, displacement and third person. These changes in language and behavior are the cues to which psychiatrists and administrators respond in their evaluations, and thus provide the basis for the clinical ratings of "recovered" and "much improved" (9).

In contrast, those subjects who are empathic and introspective, who are not rigid, conventional or stereotyped, and who rely primarily on verbal patterns of communication are less likely to manifest the ludic behavioral modes of euphoria and hypomania. With the induced alteration in brain function they manifest increased somatization, withdrawal, projection, anxiety, panic and intellectualization. Their speech is predominantly in the present tense and in the first person mode without displacement, denial or clichés. Clinically, such patients are rated as "unimproved" or "worse."

Thus, while altered brain function is essential for a behavioral change in convulsive therapy, individual differences in personality organization provide the basis for the variability in the types of behavioral changes and in the clinical ratings of improvement. In another report (12) it was suggested that the personality attributes which provide the background for improvement with convulsive therapy also provide the basis for the depressive adaptation initially. It was noted that numerous authors had described a characteristic pre-depressive personality type, with a prominence of the features of perfectionism, rigidity, conscientiousness, and stereotypy. The social factors, Rorschach and F scale patterns described here also support such a suggestion. Ludic patterns of depression and mania are more prominent in older, less educated subjects. The conventionality, rigidity and stereotypy associated with the findings on the F scale and the Rorschach test are also prominent in depressive illnesses. It is probable that the depressive psychotic reaction and the euphoric-hypomanic behavioral response in convulsive therapy may be aspects under different neurophysiologic conditions of an adaptive pattern in subjects with a personality organization marked by stereotypy, rigidity, conventionality and poor capacity for introspection and empathy.

CONCLUSION

In studies of convulsive therapy, differences in personality organization and sociologic aspects of history have been related to differences in behavioral response. Persons who are stereotyped, rigid, non-empathic and non-introspective, as defined by explicit criteria in Rorschach, F Scale and structured family interviews, and who are less educated, older and foreign born are more likely to manifest behavioral modes of euphoria and hypomania and to be evaluated as "recovered" and "much improved." Patients who are introspective, empathic, non-stereotyped, native born, better educated and young are more likely to manifest somatization, paranoid-withdrawal and panic modes of behavior with convulsive therapy, and to be rated as "unimproved" or "worse."

While an induced alteration in brain function is necessary for behavioral change in the convulsive therapy process, personality

organization and sociologic factors are determinants of the type of behavioral change, and of the clinical ratings of degree of improvement.

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STUDIES ON SERUM OXIDASE (CERULOPLASMIN) INHIBITION BY TRYPTOPHAN METABOLITES[1]

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INTRODUCTION

There have been several reports in the literature which state that the oxidase activity of serum taken from acute schizophrenic patients is higher than that of normal serum (1, 3, 4, 14, 18, 19). Other studies have appeared which are not in agreement with these findings (8, 9, 10, 17, 23). The serum oxidase activity was shown to be due to the copper protein called ceruloplasmin (4, 15, 16, 18). Apparently the reaction is complicated, being very dependent on pH, temperature and substrate (6, 12, 15), as well as being affected by various substances (3, 11, 16) including ascorbic acid (3, 4, 5, 9, 10, 17). However, the variables can be controlled and the original assay (4), with modifications to yield conditions which permit assaying the oxidase activity at optimum and constant conditions, can be employed (6).

Because it has been shown that ceruloplasmin levels in blood can fluctuate and be elevated in certain disease states as well as stress states (2) it was decided to study the effect of certain normal metabolites on this enzyme system *in vitro*. This paper is concerned with the effect of 5-hydroxyindole compounds and other tryptophan metabolites.

EXPERIMENTAL

The reaction was carried out in a temperature controlled (25 ± 0.1°C) Beckman Spectrophotometer. The pH and substrate concentration of the reaction mixture was kept at optimum values. The substrate used was N,N-dimethyl-p-phenylenediamine dihydrochloride (DPP) which was obtained from Eastman Kodak Company.

Rabbit serum was obtained by separating it from blood drawn via a cardiac puncture. The rabbits weighed approximately 2 kg. and were albino. A limited amount of ceruloplasmin was supplied by Ortho Research Foundation. Human serum was obtained from staff members as well as from patients at the V. A. Hospital, Indianapolis, Indiana. Serotonin creatinine sulphate was a gift from Abbott Laboratories.

The test system used was the following: to 1.5 ml. of serum in a Beckman cuvette, 0.2 ml. of the compound under investigation was added followed by 1.5 ml. of 0.35% DPP solution (0.3% DPP in experiments using rabbit serum). The mixture was rapidly stirred with a thin polyethylene stirrer and the optical density read at 552 mμ against a blank containing 1.7 ml. water plus 1.5 ml. serum. Measurements were made at varying time intervals up to approximately 10 minutes. In the experiments with purified ceruloplasmin, 1.3 ml. of U. V. irradiated serum (no oxidase activity present) plus 0.2 ml. ceruloplasmin were substituted for 1.5 ml. serum. The oxidase activity of purified ceruloplasmin or serum can be destroyed by U. V. irradiation (7).

RESULTS

In Figure 1 data are presented to show the effect of different concentrations of serotonin on the *in vitro* oxidation of DPP by rabbit serum at optimum conditions. The reaction is inhibited depending on the concentration of serotonin. If the difference in optical density between the control and each experimental sample 500 seconds after

[1] A preliminary report of a portion of this study was presented at the Federation Proceedings, Philadelphia, Pennsylvania, April, 1958.

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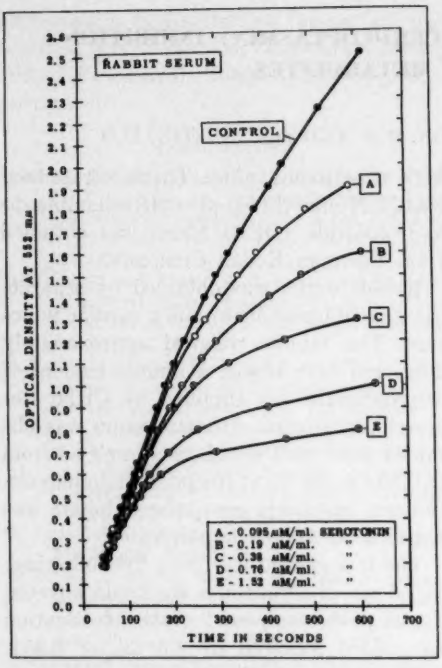

FIG. 1. The effect of different concentrations of serotonin on rabbit serum oxidase. The pH of the reaction mixture was 6.35; substrate concentration was 7.2×10^{-3} M. Reactions were run at constant temperature (25 ± 0.1°C.) in a Beckman Model DU Spectrophotometer.

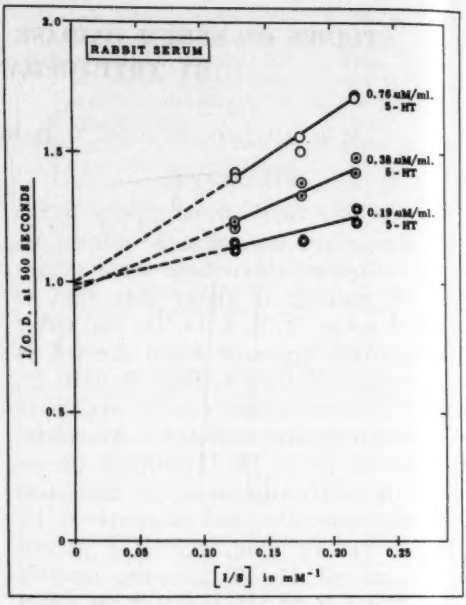

FIG. 2. Competitive inhibition of DPP by serotonin.

the start of the reaction is plotted against the log of the serotonin concentration, a linear relationship is obtained through 0.76 μM/ml. (micromoles per milliliter or mM) serotonin (11). If the substrate concentration is varied along with the serotonin content and the data are plotted according to Lineweaver and Burk (20) as shown in Figure 2, it appears that serotonin is acting as a competitive inhibitor. Note that an increase in the slope of the $[1/OD]_{500\ sec.}$ vs. [1/S] plot is accompanied by no significant change in the Y intercept. The "best" straight line was determined by the method of least squares.

Data of a typical experiment on the effect of serotonin on the *in vitro* oxidation of DPP by human serum are presented in

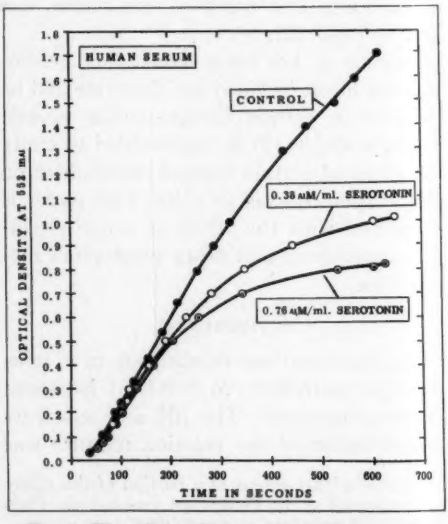

FIG. 3. The effect of serotonin on human serum oxidase. pH was 6.2, substrate concentration was 8.4×10^{-3} M. The reaction was run at 25 ± 0.1°C. in a temperature controlled Beckman Model DU Spectrophotometer.

Figure 3. In Figure 4, the effect of serotonin and 5-hydroxyindole-3-acetic acid (5-HIAA) on the *in vitro* oxidation of DPP by purified human ceruloplasmin are shown. The greater effect of 5-HIAA as compared to 5-hydroxytryptamine was also noted with human serum but not with rabbit serum. In the case of the latter the effects were comparable.

When 5-HIAA was checked to see if it was acting as a competitive or non-competitive inhibitor, it was found to be the latter. The data are presented in Figure 5. The "best" line was determined as above by the method of least squares. It is readily seen that both the slope and the Y intercept increased with increasing concentration of 5-HIAA.

Thirteen compounds thought to be involved in the metabolism of tryptophan were studied to see what effect, if any, they had on the reaction. These data are presented in Tables 1 and 2; only the data in which the added substance was present at a concentration of 0.38 μM/ml. are given. From Table 1, it can be seen that the greatest inhibition was obtained with 5-hydroxyindole-3-acetic acid and 5-hydroxytryptophan. No apparent difference in the effect of a specific compound on the oxidase activity in serum taken from hospitalized schizophrenic patients, hospitalized but non-schizophrenic patients and normals were noted in these experiments. Eight of

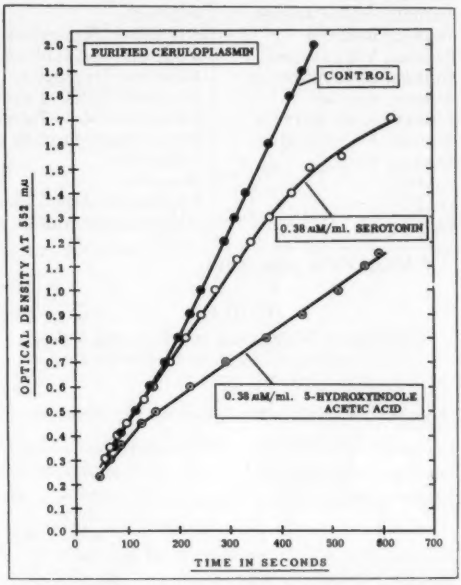

FIG. 4. The effect of serotonin and 5-hydroxyindole-3-acetic acid on purified human ceruloplasmin (50%). Reaction parameters as in Fig. 3.

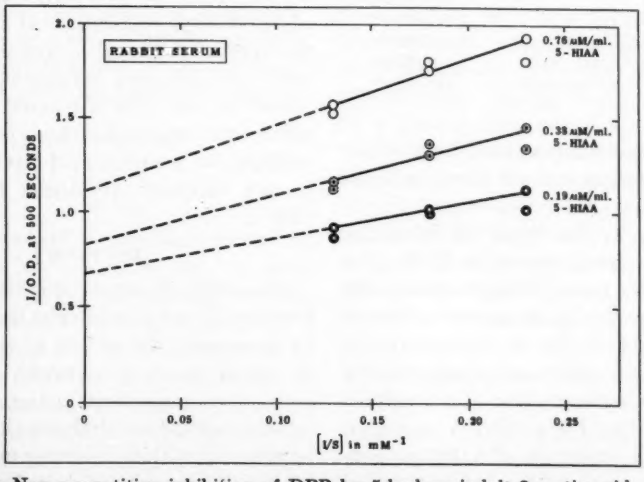

FIG. 5. Non-competitive inhibition of DPP by 5-hydroxyindole-3-acetic acid.

TABLE 1

The Effect of Several Tryptophan Metabolites on the Oxidation of DPP by Serum Oxidase

Serum Enzyme Source	Substance Added (0.38 μM/ml)	Determinations	Per Cent of Control at 400 seconds*
human, normal	serotonin	7	76.8 (68.7–84.0)
human, VA patients	serotonin	10 (pooled)	73.2
human, schizophrenic	serotonin	5	74.2 (66.7–81.3)
human, normals	5-hydroxytryptophan	6	60.8 (51.4–65.0)
human, VA patients	5-hydroxytryptophan	10 (pooled)	53.3
human, schizophrenics	5-hydroxytryptophan	6	61.8 (54.6–85.5)
human, normal	5-hydroxyindole-3-acetic acid	3	56.9 (50.0–62.3)
human, schizophrenic	5-hydroxyindole-3-acetic acid	3	52.4 (49.4–72.4)
human, VA patients	3-hydroxyanthranilic acid	10 (pooled)	84.0
human, normal	bufotenine	2	81.9 (78.4, 85.4)
rabbit	serotonin	4	76.1 (62.7–84.6)
rabbit	5-hydroxyindole-3-acetic acid	2	76.8 (74.6, 79.0)
rabbit	3-hydroxyanthranilic acid	1	80.4

* Mean value plus range.

TABLE 2

Tryptophan Metabolites which do not Affect the Oxidation of DPP by Serum Oxidase

Serum Enzyme Source	Substance Added (0.38 μM/ml)	Comment
human	tryptophan	no effect
human	anthranilic acid	bowing of curve after 400 sec.
human	indole-3-acetic acid	no effect
human	indole-3-ethylamine	no effect
human	3-hydroxyindole	no effect
human	indole-3-propionic acid	no effect
human	nicotinamide	no effect
rabbit	indole	no effect
rabbit	tryptophan	no effect
rabbit	anthranilic acid	no effect
rabbit	indole-3-acetic acid	no effect
rabbit	indole-3-propionic acid	no effect

the thirteen compounds studied have no effect on the reaction and are listed in Table 2.

In addition to the type of inhibition shown by compounds listed in Table 1, a second type was found. Compounds of this latter group are similar in action to that of ascorbic acid (4, 9, 10). A reaction occurs between the compound and oxidized DPP preventing the accumulation of the colored substance (10, 13). The result is a lag period in the *in vitro* oxidation of DPP proportional to the concentration of the added compound. If the measure or unit of enzyme activity is taken as the optical density reading after a finite length of time, the result is a lower value, indicating inhibition. However, after the substance is used up (after the lag period) the reaction takes off and the slope can be the same as the control or different. In measuring the serum oxidase or ceruloplasmin oxidative activity, the slope is proportional to the enzymatic activity. In Figure 6 data are presented to show the effect of 5-hydroxyanthranilic acid on this oxidation reaction. The slope of the curves at the two higher concentrations of 5-hydroxyanthranilic acid are lower than the control. Although 5-hydroxyanthranilic acid was the only tryptophan metabolite tested to date which reacted in this way, other non-tryptophan-like compounds in addition to ascorbic acid have been found to act similarly (cysteine, penicillamine, (13)).

DISCUSSION

The study of enzyme inhibition offers the biochemist and physiologist the opportunity to investigate the nature of the active site at which the enzyme works and the possible mechanism of its action. It also provides a method for studying the functioning of enzymes within the living system. An inhibitor is capable of interfering with an

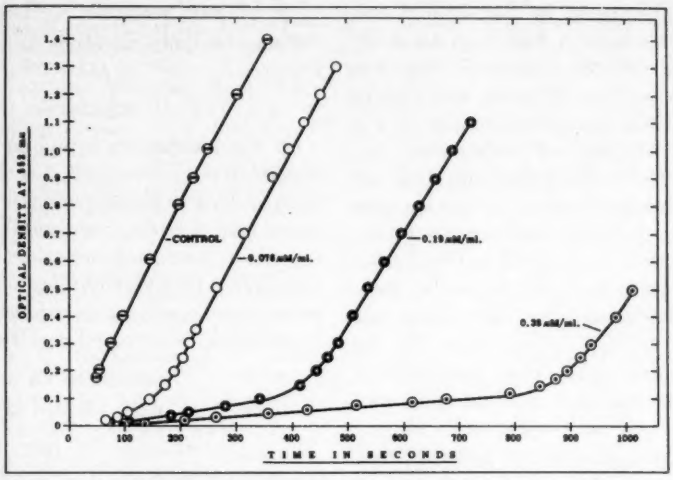

FIG. 6. Effect of 5-hydroxyanthranilic acid on the oxidation of DPP by human serum oxidase.

enzymatic reaction in several ways. In this study the two more probable mechanisms in operation appear to be a) combination with the protein-active groups—those that normally combine with the substrate, and b) reaction with the substrate.

Of the five tryptophan metabolites studied to date which inhibit the oxidation of N,N dimethyl-p-phenylenediamine by serum oxidase, four were 5-hydroxyindole derivatives. The exception was 3-hydroxyanthranilic acid. The common grouping in 5-hydroxytryptophan, serotonin, 5-hydroxyindole-3-acetic acid and bufotenine is:

When the hydroxyl group was removed as in the case of the analogs of the first three compounds, namely, tryptophan, tryptamine, and indole-3-acetic acid, and these substances were added in comparable doses, no effect was observed on the oxidation of DPP by serum oxidase. Indole itself had no effect on the reaction. It appears that the location of the hydroxyl group in position 5 is also important for this reaction to take place. If it was moved to the 3 position as in 3-hydroxyindole, the inhibition effect was not noted. Unfortunately, it was not possible to obtain other hydroxylated indole compounds to study.

When the R structure of the compounds tested contained a carboxyl group, as in 5-hydroxyindole-3-acetic acid and 5-hydroxytryptophan, the greatest inhibition occurred. When the substance under study contained a primary amine group as in serotonin the effect was less pronounced while bufotenine, in which both hydrogen atoms on the amine nitrogen are replaced by methyl groups, had the least effect.

The surface of the protein molecule, ceruloplasmin, must have specific sites which permit 5-hydroxyindole type compounds to hinder the DPP molecules from forming an enzyme-substrate complex, resulting in fewer becoming oxidized. When the 5-hydroxyindole derivative also contains a carboxyl group the interaction with the enzyme surface apparently is more "complete" producing a more effective block.

In the case of anthranilic acid derivatives, the hydroxyl group as well as its position on the nucleus is again important. The

3 position appears to be the critical one. When anthranilic acid was used to study its effect on the reaction, inhibition was not noted. When the OH group was present at the 3 position the effect produced was comparable to that of bufotenine. Although 3-hydroxyanthranilic acid does not contain an indole nucleus, it does possess an hydroxyl, carboxyl, and amino group. When the hydroxyl occurred in the 5 position, the inhibition of the oxidation reaction was delayed and the lag period was proportional to the concentration of the substance added. The latter phenomenon was similar to that seen with ascorbic acid and cysteine (4), and is known to be due to an interaction of inhibitor and substrate (13). It may be of interest to point out that tyrosine and tyramine at comparable concentrations did not have any effect on the oxidation of DPP by serum oxidase.

In 1957, Porter *et al.* showed that ceruloplasmin purified from human serum could oxidize serotonin over a three hour period (22). Recently Martin *et al.* demonstrated that human serum oxidase could oxidize serotonin (assayed over a 8–9 hour period) but not 5-hydroxytryptophan, 5-hydroxyindole-3-acetic acid and tryptophan (21). In Figure 2 data are presented to show that in the system employed in this study, serotonin was acting as a competitive inhibitor of DPP in the presence of ceruloplasmin during the 10 minute period of the experiment. The possibility existed that serotonin was acting as a competitive substrate rather than a competitive inhibitor. However, preliminary experiments involving a) incubation of serotonin with serum for different lengths of time prior to running the reaction with DPP, b) analysis of aliquots for serotonin taken from a reaction that was under way and containing serum, serotonin and DPP, and c) taking aliquots from such a mixture and running chromatograms (butanol, acetic acid, water) followed by spraying with the indole reagent, p-dimethylaminobenzaldehyde, all suggest that very little if any serotonin is being oxidized during the short duration of these experiments.

SUMMARY

Of the thirteen tryptophan metabolites studied to date, serotonin, 5-hydroxytryptophan, 5-hydroxyindole-3-acetic acid, bufotenine and 3-hydroxyanthranilic acid were capable of inhibiting the oxidation of N,N-dimethyl-p-phenylendiamine by purified ceruloplasmin and serum oxidase. Two types of inhibition were noted and discussed.

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A COMPARISON OF THE EFFECTS OF INVESTIGATORS' AND THERAPISTS' ATTITUDES IN THE EVALUATION OF TRANQUILIZERS PRESCRIBED TO HOSPITAL PATIENTS

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INTRODUCTION

In recent years a large number of psychopharmacological agents have been introduced into psychiatric practice (6, 8, 9, 14). These drugs have been employed in a variety of clinical settings with reports of their effectiveness varying from enthusiastically positive endorsements to cautious and negative findings (1, 4, 12, 17). The divergent opinions have helped to stimulate interest in the theoretical and experimental approaches to the evaluation of results of drug therapy (3, 7, 9, 10, 11, 12).

It has become almost a cliché to state that the attitudes of the investigator or evaluator play a significant role in his assessment of results of therapy (5). Most observers agree that complex transactions occur between the evaluator, the social milieu, the personality of the patient, and the pharmacodynamic effects of a particular medication (2, 11, 15, 18). Not only do these factors affect the results of medication, but they are also integrally involved in the process of evaluation of results. The present report describes some of the aspects of this complex field and provides specific data for analysis. It discusses the effects of differences in attitudes and differences in observational viewpoints upon the evaluation of results of pharmacotherapy.

The psychiatric section of Michael Reese Hospital is well suited to such an investigation. A very large staff of private attending psychiatrists (over 100) with diverse training and background treat patients at this institution, each patient having his individual psychiatrist. A large number of the staff are psychoanalytically oriented and tend to employ psychotherapy as the treatment of choice, while others employ a wider range of therapies, including EST and milieu therapy. In addition to the attending staff, a relatively high ratio of administrative and nursing personnel work intensively with the patients and independently evaluate their progress. Consequently the setting is well suited to provide data to answer the following two questions: 1) To what extent do different attitudes toward the use of tranquilizers affect the therapists' evaluation of the results of the total treatment? 2) How do therapists compare with other observers in their evaluation of the contribution of the tranquilizer medication?

METHOD

The hospital course of every patient prescribed tranquilizers[3] was followed by the investigators. Each week anecdotal material was gathered from nurses, aides, residents and occasionally attending psychiatrists about the status of the patient; changes in mood, attitude, affect, verbalizations and activities were noted. At the end of each month the material was summarized and the patients classified by the investigators on an eight point scale (see Table 1) designed to rate the effect of the drug on the course of the illness (12).

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[3] Tranquilizers were defined for the purposes of this study as those newer, non-barbiturate, ataractic drugs which were used at this hospital during the period of the investigation (for the most part chlorpromazine, meprobamate, and promazine).

Each patient was given a final rating on discharge from the hospital or discontinuation of tranquilizer prescription; the rating reflected an overall summation of the effect of the drug on the treatment. The individual psychiatrists who treated the patients and prescribed the drugs were asked to rate results of therapy on the same eight point scale. This rating was based on the entire course of treatment of each patient, both in and out of the hospital[4]. The psychiatrists were then asked to rate themselves on a four point scale which was designed as a gross estimate of their attitude toward tranquilizer medication. The investigators also rated themselves.

TABLE 1

Rating Scale for Patients Receiving Tranquilizing Drugs

1. Patient improved; strong evidence that the drug facilitated the improvement.
2. Patient improved; role of the drug not clear.
2a. Patient improved; strong evidence that the improvement was independent of the drug.
3. Patient remained about the same.
4. Patient became worse; role of the drug not clear.
4a. Patient became worse; strong evidence that the deterioration was independent of the drug.
5. Patient became worse; strong evidence that the drug contributed to the deterioration.
6. Patient could not be placed in any category.

TABLE 2

Comparison of Therapists' and Investigators' Opinion regarding Tranquilizers' Role in Patient Improvement

Attitude toward Tranquilizer Drugs	Group	No. of Therapists	Total No. of Cases	No. of Cases agreed to have improved (by Ther. & Inv.)	Categorization of Improved Cases		
						1 and 2	2a
Are of little help	I	11	34	15	Inv.	15	0
					Ther.	3	12**
Provide only relief of symptoms	II	43	202	110	Inv.	101	9
					Ther.	63	47**
Provide relief which promotes treatment	III	5	96	50	Inv.	42	8
May be crucial therapeutic agents		10			Ther.	32	18*
		15					

* level of significance $<.05$
** level of significance $<.001$

During the forty-eight week period of the study three hundred and thirty-eight patients, approximately forty per cent of the total hospital admissions, were prescribed tranquilizers by seventy psychiatrists. Sixty-nine psychiatrists completed the questionnaire, rating three hundred and thirty-two patients; of these patients, the psychiatrists rated seven as not classifiable (category six); the investigators rated sixteen as not classifiable, largely due to insufficient time for observation. This paper is largely concerned with comparing ratings of the investigators with those of the therapists.

[4] The private psychiatrists rated many of the patients after their hospital course was completed. Simultaneous ratings, although desirable, were not possible in this study.

RESULTS

ATTITUDE OF THE THERAPISTS

Eleven of the sixty-nine psychiatrists rated themselves on the four point scale as believing that tranquilizers were of little help in the treatment of their patients. These are hereafter referred to as the Group I therapists. Forty-three psychiatrists rated themselves as believing that the tranquilizers were effective in providing relief from symptoms but did not markedly alter the course of therapy. These are referred to as

the Group II therapists. (The investigators independently rated themselves as in this group.) There were five psychiatrists who rated themselves as believing that the tranquilizers were effective in providing relief from symptoms and altering the course of therapy, and there were ten who felt that tranquilizers may have been the crucial therapeutic agent in some patients' improvement. For statistical purposes the last two groups are combined as the Group III therapists; those most favorable to the use of tranquilizers.

COMPARISON OF INVESTIGATORS' AND THERAPISTS' RESULTS

The eleven therapists in Group I, who believed tranquilizers to be of little help in the treatment of patients, did prescribe tranquilizers in thirty-four cases. Of these cases the investigators and therapists agreed that fifteen patients were improved (rated as category one, two or two-a). The therapists excluded the drug as a factor in the patients' improvement in twelve of the fifteen cases (rating two-a). The investigators included the drugs as possible factors in patient improvement in all fifteen cases. As a matter of fact, it was their opinion that the drug facilitated improvement in seven cases (category one). Chi-square analysis indicates that this difference is significant at a level of less than .001. Of the 202 patients treated by the forty-three psychiatrists who rated themselves in Group II there was agreement to patient improvement in 110 cases. The investigators believed that improvement was related to drugs in 101 of the 110 cases, while the therapists felt that improvement was related to drugs in 63 cases and unrelated in 47 cases, a difference also significant at a level of less than .001. The fifteen psychiatrists most favorable to pharmacotherapy treated 96 patients with tranquilizers. Of these cases, the investigators and the therapists agreed that there was improvement in fifty patients. The investigators considered that the improvement was drug related in forty-two cases and unrelated in eight cases, while the therapists felt drugs were related to improvement in thirty-two cases and unrelated in eighteen. Here chi-square yields an index significant at less than the .05 level. Note that the investigators include the tranquilizers as playing a role in patient improvement consistently more often than do all of the therapists.

COMPARISON OF RESULTS AMONG THERAPISTS

The three groups of therapists did not differ significantly from each other in the percentage of patients stated to have improved. Group I reported fifty-nine per cent improved, Group II felt sixty-nine per cent improved, and Group III felt sixty-seven per cent improved. However, there were differences in terms of what the various therapists related the improvement to. Comparison of Group I and Group III therapists' ratings showed that the latter tended much more frequently to relate improvement to drug use than did the former (chi-square is significant at a level of less than .01). Obviously, in this analysis the two groups of therapists were not rating the same patients; however only those on whom there was agreement of improvement by both therapist and investigator were included. There was a lesser but still highly significant difference (at a level of less than .02) between Group I and Group II therapists; those less favorable in attitude to tranquilizer prescription less often related improvement to the use of drugs. The difference between the Group II and Group III therapists in this regard was not significant.

These comparisons demonstrate that therapists who considered tranquilizers to be of little help in the treatment of their patients rated improvement as being unrelated to drug administration more frequently than did those who were favorably disposed to tranquilizer therapy.

A surprising additional finding involves patients who were judged to have become

worse during the course of tranquilizer therapy. The therapists considered that the condition of twenty-eight patients had deteriorated, while the investigators classified ten patients in this way. However, only in one case did the therapists and investigators agree in placing a patient in this category—an impressive disagreement as to what constitutes behavior that may be called "worse."

CLINICAL EXAMPLES

It is hoped that some clinical examples will clarify how differences between therapists and investigators result in different ratings.

Case A. For several weeks this thirty-five year old male acute schizophrenic patient was treated on the maximum security unit. He attempted to escape several times, refused to see his physician, insistently demanded to be released and refused to eat. The hospital personnel became increasingly disturbed by his behavior, thus lessening their ability to deal with him. Gradually there developed an increasing pressure on the therapist to prescribe tranquilizers. When the personnel felt that they could no longer handle this patient adequately, his therapist placed him on tranquilizer therapy. Immediately the patient responded by becoming much calmer, more controlled and less assertive in demanding his release from hospital. Thus, from the point of view of ward behavior the drug was remarkably effective. However, the therapist, who was himself negatively disposed toward tranquilizer treatment, rated this patient as becoming worse as a result of pharmacotherapy. The therapist felt that the patient saw his doctor as trying to control rather than trying to work with him and because of this the therapist believed that the psychotherapeutic relationship was seriously hindered. The investigators, largely responding to the personnel's reaction, differed from the therapist and rated the patient in the category of improvement facilitated by pharmacotherapy.

Case B. This forty-eight year old paranoid psychotic woman required psychiatric hospitalization despite intensive psychotherapy and chlorpromazine. The therapy was continued after her admission to hospital and for a short period the patient became calmer and was more accepting of reassurance from personnel. After about five weeks her status began deteriorating and the drug was discontinued as being ineffective. Shortly afterward it was learned that the patient had been hiding instead of taking the drug for two weeks prior to its discontinuation; however, medication was not reinstituted at this point. The patient became more disturbed but instead of transferring her to a more secure unit the administrative psychiatrist on call decided on another trial of medication. The patient seemed to respond positively to this, in that the ward personnel felt that she was able to remain on the less disturbed unit. However, the attending psychiatrist, because of his previous experience with the drug with this patient, thought the medication would not provide effective results, ordered her transferred to a more secure unit and initiated EST. In this situation the investigators reported improvement from medication while the therapist reported the tranquilizer as ineffective. This physician was unfavorably disposed to tranquilizers (Group I).

Case C. This case is in contrast with the first two reported. The patient was a twenty-nine year old single woman who had had several previous admissions to the hospital, having been diagnosed as a chronic schizophrenic patient subject to exacerbations and remissions. In the hospital she was placed on drugs and her behavior showed a slow steady improvement. The investigators rated her as being improved but felt that the role of the drug was not clear. The therapist, who had seen her through several such episodes, felt that her improvement was more marked on this admission than on

previous ones. He attributed this difference to medication and rated the improvement as being definitely related to the drug administration. This physician's attitude to drugs was that they were effective in relieving symptoms and could also alter the course of treatment. In this instance the rating difference between the therapist and the investigators is opposite to the main trend as noted above.

DISCUSSION

This study has illustrated that pre-formed attitudes toward tranquilizer drugs affect psychiatrists' judgment of the role of a drug in patient improvement. It is important to note that our findings show that Group I, II, and III therapists do not differ significantly from each other in the percentage of patients on medication stated to have improved. Therapists do differ in the *explanation* of the improvement, and this difference is clearly related to their expressed attitudes. That the attitudes are pre-formed and not a direct result of the experiences with these patients is evident from the fact that all groups achieved approximately equal results.

Although presently not feasible, it would be of value to make an equivalent study of that segment of the hospital population (sixty per cent) which was not prescribed tranquilizers; and to conduct similar studies of percentage of improvement and explanations of improvement by psychiatrists and investigators.

A second finding is the consistent and significant difference between therapists and investigators, in their respective judgment of the role of drugs in patients' improvement. Why do the therapists exclude the tranquilizers as playing a role in patient improvement so much more often than do the investigators? Conversely, why do the investigators tend to ascribe a greater importance to the tranquilizers in patient improvement?

Among possible explanations the following considerations should be noted. It is important to recall that the therapists rated retrospectively, while the investigators made their ratings concurrent with their observations of the patients. Ideally it would have been better for investigators and therapists to make ratings at the same time. The major finding that could be explained by this difference in time of rating is the disparity noted in rating worsening conditions of patients. Since the therapists made their observations over a longer time span they may have treated some patients who deteriorated after leaving the hospital. However, this could not fully account for the discrepancy in findings. Nor does the lack of simultaneity of ratings account for the differences in factors cited as responsible for patients' improvement (in cases where there was agreement in patient improvement).

It is apparent that the scales employed for measuring attitudes towards tranquilizers in this study distinguish broad categories only; thus individuals may have rated themselves within the same group for a variety of ideological and attitudinal reasons that were obscured by the scales. For example, sociotherapeutically oriented and psychotherapeutically oriented psychiatrists would not be differentiated by this classification (16). Nevertheless, it is impressive that positive results were obtained with such a scale. As more refined methods to study attitudes of psychiatrists are developed and validated it will be possible to repeat this study in a more precise fashion.

We believe that the best explanation for the difference in the role ascribed to tranquilizers by the investigators and the therapists involves the differing observational fields of the two groups. It is clear that the investigators were hospital-oriented, utilized social behavior of the patient as a key factor in rating, relied heavily on ward personnel's observations, and viewed the doctor-patient transaction as one part of the total field. We viewed improvement from an interpersonal and social context and had relatively

little intrapersonal information. The therapists quite understandably tended to focus on their individual contact with one patient and often saw the medication and the hospital setting as ancillary to their own therapeutic efforts. Improvement was defined in intrapersonal as well as behavioral terms, and several patients were rated to have become worse who showed social improvement. This was most clearly true of Group I and most of the Group II doctors—as has been illustrated in the clinical examples. It was also true among Group III therapists, several of whom, despite their very positive statements regarding tranquilizers, indicated that they tended to rate the drugs as less important than their personal contact with the patient. Others of Group III tended to attribute improvement primarily to electroshock or other somatic therapies.

CONCLUSIONS

This study has reported three findings.

Therapists differ in the way that they evaluate the role of tranquilizer drugs in patient improvement according to pre-formed attitudes.

Irrespective of their attitudes, therapists as a group differed significantly from the investigators in their evaluation of the role of tranquilizer drugs in the improvement of patients.

Investigators and therapists differ significantly, *i.e.*, use different criteria, in adjudging a patient's condition as worsening.

Previous workers (5, 13) have indicated that the attitude of the physician prescribing tranquilizer medication affects the evaluation process, and our findings clearly support this hypothesis. The major implication of this paper, however, is that studies which take cognizance of positive or negative attitudes toward drugs may only partially explain the variable results reported by different workers. The striking differences between the investigators and the therapists in this study highlight the importance of determining the observational position of the evaluator in the rating process.

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EPILEPSY AND THE LAW

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INTRODUCTION

Epilepsy is a disease with many legal ramifications about which the physician will often be asked questions, by patients and their families as well as by attorneys and others. The purpose of this paper is to review this general topic of epilepsy and the law.

As the disease of epilepsy is one which has been frequently encountered through the centuries, it has a long social history. Thus today, many of the attitudes toward this formerly "sacred disease" reflect feelings more appropriate to a period when knowledge of the disease was quite limited. These very attitudes have become hardened and fixed into our cultural pattern by a legal system which has singled out epilepsy both in common law and by statute. As a result, there are numerous laws which affect the epileptic in relation to marriage, sterilization, driving, Workmen's Compensation, and mental responsibility both in criminal and in civil cases. Comments will be made concerning legal definitions of epilepsy, the importance of medical classifications, incidence and course, heredity, and some aspects of electroencephalography. The rationale of existing laws will be discussed and illustrative legal cases will be presented.

DEFINITION OF EPILEPSY

Statutory laws regarding epilepsy do not define the term (16), or if they do, it is to state, for example (25), that an epileptic is one who has epilepsy. The definition found in New York law (19) is a practical one—an epileptic is "a person suffering from epilepsy as defined in medical practice." Although lawyers usually are quite explicit in defining words, a general definition here of a scientific term is most meritorious, as it allows the physician to determine the application of the word and it also allows for changes in medical thinking without necessitating a change of statute. The word epilepsy is derived from the Greek word *epilepsia*, meaning seizure, and though "fits" and "seizures" are often used synonomously with epileptic attacks, they lack a specificity necessary in law. The point of specificity is important since being called "epileptic" is a great handicap to a patient in many connections, and thus the designation should be kept as narrow as possible. In addition to laws which apply specifically to "epilepsy," many laws apply to a group of conditions which include epilepsy but do not mention it by name. For instance, the Ohio driving code (23) refers to "any condition resulting in episodic impairment of consciousness or loss of motor control"—a definition which would include such illnesses as hypoglycemia or recurrent fainting due to any cause. For the purpose of screening out driving risks, a broad definition is more pertinent and useful.

Epilepsy might be described as a disease or symptom complex characterized by recurrent alterations in the state of consciousness and/or episodic abnormal muscular movements, accompanied by an electrical discharge in the brain, with certain exceptions. This definition stresses certain elements; the recurrent aspects, the change in consciousness, the electrical discharge, and the words, "with certain exceptions." The last is most important since other diseases may approximate the rest of the description but are not considered to be epilepsy. Such conditions as hypoglycemia, hysteria, and drug intoxication are examples. Merritt (17) defines epilepsy as a "functional disorder characterized by recurrent attacks of loss of consciousness, with or without convulsive

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movement," and most neurological textbooks have similar definitions. The central issue here is that many definitions are not exclusive, and such a point must clearly be made in a court of law. Lennox (11) has stated, "Epilepsy is simply a paroxysmal dysrrhythmia of brain potentials." While scientifically accurate, this frame of reference is also too broad and would be quite misleading and confusing if used in courts, since there can be paroxysmal dysrrhythmia without epilepsy.

The question may arise in court as to whether epilepsy is a disease or symptom. This is academic in that it is both, but if it is a symptom, knowledge of the cause is obviously of great practical medical and legal value.

TYPES OF EPILEPSY

There are several classifications of epilepsy and these will be mentioned because of differences in legal significance in different types. This is especially important in the subject of heredity and marriage.

Epilepsy is traditionally divided into two main categories, (1) idiopathic and (2) symptomatic. Until the last few decades, not much was known of the etiology of epilepsy. Where there was no apparent cause (and this was in the vast majority of cases), terms such as "idiopathic," "cryptogenic," "genuine," or "essential" epilepsy were used. Where the physician could attribute the symptoms to some specific organic disease, the term, "symptomatic" was used. Essential epilepsy constitutes about seventy-eight per cent of cases. Today this distinction is becoming blurred as predisposition, familial dysrrhythmias, thresholds, and other concepts have made such separation less clear. But for practical and historical purposes, the continuation of this classification is merited.

Idiopathic epilepsy is most likely to arise in the first twenty-five years of life. Although few specific pathologic findings have been demonstrated with resultant classification as a "functional" disease, histopathology has demonstrated some findings similar to those found in anoxia; and the electrochemical patterns have been widely investigated. In symptomatic or secondary epilepsy, the seizures are the result of some other basic change in the brain, and the course, treatment, and social consequences depend on this latter factor. These disease processes cover almost the whole scope of neurology; Alpers (1) in his textbook lists sixty-seven causes of symptomatic epilepsy. Early in life, one encounters such diseases as congenital malformations, birth injuries, neoplasms, and infectious processes; later in life head injuries, neoplasms, cerebral arteriosclerosis, and other vascular diseases play an important role. Thus investigation as to possible cause is most important in handling any case of epilepsy. Tables 1 and

TABLE 1

*Recurrent Convulsive Seizures in 2,000 Non-Institutional Cases of Epilepsy at All Ages.**

Presumed Causes of Seizures	%
None demonstrated	77.6
Cerebral trauma	5.7
Birth injury or congenital defect	5.6
Brain infection	4.2
Brain tumor	2.6
Cerebral circulatory defect	1.9
Extracerebral causes	0.9

* After Lennox (11).

TABLE 2

*Analysis of 689 Patients Whose Attacks Began After 20 Years of Age**

Category	%
Idiopathic epilepsy	77.2
Hypertension or cerebral arteriosclerotic	12.1
Alcohol	4.1
Post-traumatic	2.5
Neurosyphilis	1.6
Pregnancy	1.2
Cerebral birth trauma	0.6
Brain abscess	0.3
Brain tumor	0.3
Cysticercosis	0.1

* After Livingston (15).

2 illustrate the distribution of the various types of epilepsy.

Epilepsy is commonly classified on the basis of clinical manifestation. Here too, different legal meanings may be attached to different types. "Grand mal" epilepsy is the type most commonly known to laymen and lawyers; accordingly, if epilepsy is to be faked or malingered, this would be the type usually chosen. Since a grand mal attack is difficult to mimic, it is relatively easily distinguished from conditions such as fainting, hysterical attacks, and tetany. Greater sophistication and learning will undoubtedly make evaluation and recognition of vaguer forms more of a legal problem. The grand mal seizure is of especial importance in consideration of mental responsibility. With acute onset of unconsciousness, accidents may occur. After a seizure the epileptic may become violent and destructive (epileptic furor) and this may be an element in homicide. Lately occurrences during epileptic fugues have brought about difficult medicolegal problems, and proof of a fugue may bring about the voiding of a contract or the dropping of a criminal charge.

Petit mal seizures are restricted mainly to childhood, and since they usually belong to the idiopathic group they are not of great importance as a sequel to head injuries. As the duration may be only ten to fifteen seconds, accidents or other sequellae are not so likely.

Psychomotor seizures (or "temporal lobe epilepsy") are more and more recognized. Here the person is often involved in some apparently purposeful activity such as dressing or undressing, running, etc.; however, the patient has amnesia for any actions during this period. It is reasonable to assume that this type of seizure will assume greater medicolegal importance because it is often related to injuries, may be an important element in civil cases, and is often difficult to define clearly.

The various clinical types of epilepsy are not exclusive, and one individual's seizures may vary as to type at different times.

Localized and "marching" seizures (the Jacksonian seizure) may be sequellae to injuries as well as reflect other brain pathology. Following injury, grand mal seizures and psychomotor variants are much more common. Petit mal variants, adversive seizures, and other rarer types need not be discussed here.

Seizures alone do not indicate epilepsy and must be considered in the context of the total clinical picture. For instance, five to seven per cent of infants have convulsions with high fever, but only twelve per cent of children who have seizures with high fever develop epilepsy (Lennox).

INCIDENCE AND COURSE OF EPILEPSY

Epilepsy is quite common, and in examination of draftees, the incidence was 0.5 per cent (a figure found in the United States, Holland, and Switzerland). It has been estimated that the number of epileptics in the United States varies from 800,000 to 1,500,000 (6). Epilepsy is not necessarily a lifelong condition; Lennox reports that 23 per cent of epileptic patients have a spontaneous remission in one to five years, and 6 per cent more have a cessation after five years.

Absolute numbers of treatment results are difficult to quote because of the many types of epilepsy and the many regimes of treatment. Probably at least half of epileptics eventually become seizure-free—either spontaneously or with treatment. Another large group will have the disease under fairly good control; apparently less than 10 per cent of epileptic patients remain severely handicapped. This is an important consideration, because most laws do not consider prognosis or change in the course of the illness.

Years ago, with little treatment available, the prospects for the epileptic were quite depressing. With frequent seizures, the epi-

leptic was isolated and feared by his fellow man. Typically he would be incarcerated in a state institution where he soon deteriorated mentally and physically. Epileptics were often included with the insane and mentally deficient in mental hygiene laws, and institutions for epileptics were common. As treatment and understanding ameliorated this gloomy outlook, the source of patients for these institutions diminished, and gradually these institutions were converted to other uses. Yet the legacy of past ideas remains. For instance, a common belief is that the epileptic deteriorates intellectually. Lennox, in a study of 2,000 patients (clinic and private), showed that 67 per cent are mentally normal, 23 per cent are slightly deteriorated, and 10 per cent are definitely deteriorated. Obviously a certain number of cases will have epilepsy in association with brain tissue destruction or malformation, and these will be reflected in mental defect. Individuals with an Intelligence Quotient below 50 usually have organic brain damage, and the symptomatic epilepsy should not be looked upon as the cause of the low intellectual functioning. The high incidence of mental disease and behavior disorders reflects emotional reactions to the illness rather than the epilepsy itself. Thus many epileptics respond to psychotherapy. Formerly one encountered the "epileptic personality," characterized by paranoid, withdrawn, hostile, and irritable tendencies; now this is looked upon more as a psychologic response to being an epileptic and the role in society which such a status necessitates. The same holds true with psychotic reactions—where a schizophrenic reaction may develop. Chronic brain syndromes may result from the organic brain damage. The basic point is that these complications are not a feature of epilepsy *per se*. Today the epileptic, still ostracized by society, unable to work and be self-supporting, limited both in heterosexual and general social contact, often shows a marked personality defect.

Nonetheless, most epileptics come to an adjustment that functionally is reasonably adequate.

THE ELECTROENCEPHALOGRAM

The electroencephalogram has become an increasingly important tool in evaluating and understanding epilepsy. It has also become quite useful in court cases for the same reasons. Another factor in its popularity in the courts is that electroencephalography is supposedly an objective procedure, something which will be proof of something, and the lawyer—often with a penchant for oversimplification—is prone to look upon the EEG as a definitive authority. This has led to situations where the EEG has been grossly misused and subsequently maligned. The difficulty lies in the lack of specificity in most EEG findings, yet some fearless interpreters of EEG's have utilized them to ascertain such traits as "criminal tendencies."

Thus it is important to make clear in a court of law that the EEG by itself does not usually make a diagnosis, and as physicians well know, it should be used as corroborative evidence interpreted by correlation with clinical data. A history of the patient together with observation of the patient is of far greater value in diagnosing disease than the isolated EEG, which can be quite misleading. This should be especially emphasized and respected in medicolegal cases.

Variations of the EEG are found in many clinical states (and in normals); thus "paroxysmal dysrrhythmia" and other common findings are an interpretive problem. Commonly EEG's are "compatible" with epilepsy, but not diagnostic. On the one hand, 15 to 20 per cent of normal people who never have had a seizure will show an "abnormality" on the EEG; on the other hand 10 per cent of known epileptics may show no EEG abnormalities. Numerous factors affecting the EEG, *e.g.* emotions, drugs, state of consciousness, age, as well as mechanical arti-

facts, must be considered. The concept of "delayed maturation" of the nervous system has been postulated for many disorders which show EEG findings. It is well known that psychopaths and delinquents frequently show EEG abnormalities. Levy and Kennard (14), in a study of 1,000 inmates with various psychologic disorders in four institutions, found that 50 per cent of the children studied showed abnormal EEG's, that 20–30 per cent of those in their twenties did likewise, and the incidence rose again after age 55. Ellingson (5) demonstrated abnormal EEG's in 50 per cent of psychopaths and 33 per cent of diagnosed schizophrenics. These findings are mentioned to illustrate the fact that many findings of EEG "abnormality" are at this point, of little clinical use.

Of course, EEG records obtained during a seizure will supply much more useful information. Grand mal (especially during a seizure), petit mal, and psychomotor seizures may present clearcut characteristic records. Here correlation may be quite good —such findings as the spiking of grand mal or localized deviations in psychomotor seizures may be most helpful. Several records may be of help in separating the immediate effects of a head injury, the chronic effects of head injury, and in clarifying an alleged post-traumatic epilepsy.

The EEG is most important in recognition and localization of many types of brain pathology. Thus, indications of a localized lesion such as neoplasm may be picked up, although the original referral might be, say, for evaluation of a head injury. For medicolegal purposes, then, thorough evaluation is necessary.

The technique of electroencephalography is delicate and must be done with care and good routine; otherwise, artifacts develop which complicate the picture. Lennox (12) has stated, "Many a person has been pronounced 'epileptic' because he 'dozed off' unobserved or because an electrode was improperly applied."

EPILEPSY, HEREDITY, AND MARRIAGE

Epilepsy is a disease with a strong hereditary element. Around the turn of this century, over-recognition of this fact plus the interest in eugenics so popular at that time resulted in the passage of many laws regarding the marriage and sterilization of epileptics. Epileptics were freely included in the list of groups prohibited from marriage. For instance, the Ohio law (22) states that "no marriage license shall be granted when either of the applicants is a habitual drunkard, epileptic, imbecile, or insane person, is under the influence of an intoxicating liquor or drug, or is infected with syphilis in a form that is communicable or likely to become communicable."

The laws regarding marriage vary greatly and have been the subject of many articles (*e.g.* 7, 8, 27) and have been most comprehensively reported in the book by Barrow and Fabing entitled *Epilepsy and the Law* (2). Seventeen states prohibit the marriage of epileptics based on the premise that epilepsy is strongly hereditary. In various states the following are crimes: 1) for an epileptic to marry (in six states), 2) for a nonepileptic person to marry an epileptic knowingly, 3) for a license to be issued knowingly and 4) for the solemnizing official to perform knowingly. In four states, such marriages are void, in four others they are voidable, and in ten other states they are probably voidable, although most will uphold a marriage except where epilepsy is fraudulently concealed by the party having it. In Michigan, an epileptic can marry if two physicians will certify that there has been a "complete cure" and that the condition will not be transmitted to offspring. In 1955, North Carolina changed its laws to apply only to epileptics with uncontrolled seizures. Formerly in Connecticut and Wisconsin, such marriages were null and void, and the children of such marriages were thus illegitimate. In states without any statutes, fraudulent concealment of epilepsy is grounds for annulment.

In a Connecticut case (38) of 1905, the attitude and thinking of that period is clearly expressed. "That epilepsy is a disease of peculiarly serious and revolting character tending to weaken mental force, and often descending from parent to child, or entailing upon the offspring of the sufferer some other grave form of nervous malady, is a matter of common knowledge, of which the courts will take judicial notice.... One mode of guarding against the perpetuation of epilepsy obviously is to forbid sexual intercourse with those affected by it, and to preclude such opportunities for sexual intercourse as marriage furnishes." The rule of law here was that one induced by fraudulent concealment to marry an epileptic, forbidden by statute to marry, is entitled to divorce on the ground of fraudulent contract.

Another application of this rule was in the case of Vendetto v. Vendetto (57). Here the defendant's wife had had epilepsy from childhood, and the husband did not learn of this condition until a year after their marriage in 1914. The husband continued living with his wife until she was confined to an institution in 1926, and it was then that he brought his successful suit for divorce. Other cases (33, 44) ruled that a husband or wife is entitled to annulment of marriage for fraud of the spouse in concealing the fact that the spouse was subject to epilepsy. In Busch v. Gruber (33), it was stated that the wife is entitled to have the marriage annulled notwithstanding consummation.

Sterilization laws exist in twenty-eight states. In seventeen states these laws apply specifically to epileptics, and in another state the statutes have been construed to include epileptics. Fourteen of the eighteen states limit sterilization to epileptics who have been committed to an institution. None of the sterilization statutes define epilepsy.

This briefly summarizes some of the laws based on the so-called heredity of epilepsy. Scientific evidence concerning this topic should next be examined to ascertain how realistic these laws are. It should be mentioned, on practical grounds alone, that the vast majority of epileptics can marry quite easily, either by denying epilepsy or by crossing a state line into one of the (majority of) states which have no such laws. As a result, the laws are essentially useless as far as their professed purpose is concerned.

Since epilepsy is not ordinarily defined in law, no distinction is made between idiopathic and symptomatic epilepsy. Thus, an individual who has had a brain tumor removed and has residual seizures twice a year, is put in the same legal category as the epileptic who has had several seizures a week since childhood. It is apparent that the hereditary element in symptomatic epilepsy will be quite low—low enough to make statutory limitation on marriage ludicrous. Another point of consideration is that these laws do not make distinctions as to how long a person has been an epileptic. If seizures terminate, when does such an individual cease being an epileptic by law? Will he legally always be an epileptic? What of the not atypical epileptic patient under good control of medication who has a seizure, say, every two years? If a soldier suffers a severe head injury and has a few resultant seizures, should he be prevented from marrying despite the fact that there is no discernible hereditary element? In such cases as these it is clear that legislative control is not scientifically justified.

The next and more complex problem is that of the idiopathic epileptic where there is no doubt of an hereditary relationship. To begin with, a process of natural selection tends to reduce the number of such cases. Three-quarters of epilepsy cases begin in childhood and in the teens. These people, handicapped socially and occupationally, and unable to support themselves, do not often marry even in the states where such marriages are allowed because they are often unable to find marital partners.

The influence of heredity has been vastly overstressed in the past. In one study of 138

marriages of epileptics with 553 offspring, a history of seizures was found in only 10 (1.8 per cent), and in another study of 161 children of 144 epileptic mothers, there were only three cases of epilepsy (30). Probably the largest study was that of Lennox (12), where his data indicate that the child of an epileptic has 39 chances out of 40 of being normal, though some dispute this figure. This report was based on a study of 2,000 patients and 12,119 relatives. This rate of 2.7 per cent is about five times that of the general population, derived from statistics of draftees, but is based on "cerebral dysrhythmia" rather than epilepsy *per se*. Lennox comments, "In a few diseases (Huntington's Chorea, for example), the evidence of heredity is unmistakable and the prohibition of children is absolute. But for most disorders of body or mind, the evidence is fragmentary and controversial, and decision regarding childbearing may be grounded more on fear than on fact."

Conversely it has been shown that not one epileptic in twenty-five has an affected parent, so that in probability terms prohibition of marriage would not be of much help in cutting down the incidence of epilepsy.

What seems to be inherited is not the disease itself, but the tendency as shown by the greater incidence of EEG variations in relatives, although they do not have epilepsy. This predisposition is illustrated by the statement, "Among identical twins only one of whom has epilepsy and antecedent brain injury, the normal twin usually possesses an hereditary dysrhythmia of brain waves" (26). Lennox (12) discusses in detail how to evaluate the tendency towards epilepsy when a doctor is consulted to advise potential parents whether or not to have children. His feelings may be summed up by his statement, "An undesirable inherited tendency to seizures may be outweighed by other desirable traits. Better an epileptic Caesar, Byron, or Van Gogh than a non-epileptic non-entity."

DRIVING AND EPILEPSY

It is apparent that any individual who is subject to attacks of unconsciousness would be a menace behind the wheel of a car. As a result, numerous laws have affected the driving rights of epileptics. For a thorough discussion of this and other statutes regarding epilepsy, the reader is again referred to the book by Barrow and Fabing (2). In all states except South Dakota, there are prohibitions and limitations on the rights of epileptics to drive. In four states limitations are placed on "epileptics," in 12 states on "adjudicated" epileptics, and in 32 other states the epileptic is not specified. However, each of the forty-seven states gives to the administrator of the driver's license law the right to deny a license to any person deemed to be an unsafe driver, the most common criterion being any physical or mental disease which would prevent a person's exercising ordinary and reasonable control over a motor vehicle. Information is obtained by questions on applications, reports from state institutions, and in seven states, cases of epilepsy must be reported by physicians. With the recognition that epilepsy can be controlled, licenses are now granted to epileptics under certain circumstances in at least 19 states, and in seven other states, control is one of the factors taken into account. At least eleven states specify a definite seizure-free period before a license is issued. This varies from one to three years in duration; other states individualize this period of time. The American League against Epilepsy (whose definition is this: "epilepsy is an episodic impairment of consciousness or loss of motor control, with or without convulsions and having the medical diagnosis of epilepsy") recommends one year as the basis for determining whether the controlled epileptic is a safe-driving risk.

To obtain a license in Massachusetts (11), the driver must be seizure-free for at least eighteen months, and other factors taken into consideration are the age of onset, type

of epilepsy and frequency, presence of auras which may serve as a warning, the time of day that the seizures occur, the degree of abnormality of the EEG, and the veracity and sobriety of the individual.

Some states require that the person be seizure-free without medication; others permit medication as long as the person stays seizure-free. In recent years Wisconsin has adopted the system that the licensing official can grant a driving license on the recommendation of the attending physician and certification that the applicant is under treatment and free of seizures (a period of two years is the rule of thumb). If the applicant is turned down, he can appeal to an administrative review board composed of a licensing official and two physicians. He may then be granted a permit for six months, when the case is re-evaluated and further permits issued. The initial experience in Wisconsin under this arrangement appears to have been excellent. Fabing and Barrow (2) report that, "statistics, as of August, 1954, on the issuance and denial of licenses under the Wisconsin procedure are as follows: The administrator has issued a license on the basis of medical certification in 280 cases and denied a license in 296 cases. Of the 296 applicants who were denied a license, 129 appealed to the board. Of these, the board granted a license to 84 and denied it to 45. It is understood that none of the persons whose licenses were granted under the Wisconsin procedure has been reported to have been involved in a motor vehicle accident." They also quote Dr. Schwade who reported, "Out of those persons who have been recommended for license by the board, only four have had their licenses again withdrawn and two of these were for conviction for operating a motor vehicle while intoxicated rather than for any recurrence of epileptic seizures." This history of the law in Wisconsin (18) is of considerable interest. In 1947, epilepsy was made a reportable disease; it became quickly apparent that about one-half the epileptics reported had a driver's license and had made false statements in order to obtain a license. At that time it was legally impossible for one to obtain an operator's license if he had ever been subject to epileptic seizures. These findings stimulated a change in the law to allow controlled epileptics to drive. As of April, 1958, only three persons have had their licenses withdrawn, ostensibly for recurrence of seizures. The disadvantage of the new law lies in the amount of paper work created as questionnaires are sent out at six month intervals. However, one benefit gained is that epileptics now seem to be much more truthful in applying for driver's licenses, and, of course, there is the great benefit obtained by those epileptics who are now legally allowed to drive.

Ohio in 1957 (23) adopted a similar law, but with a seizure-free period of at least one year. In the new law, the word "epileptic" was dropped, being replaced by "any condition resulting in episodic impairment of consciousness or loss of muscular control," thus also covering diseases other than epilepsy. The review board shall give due consideration "to the extent and duration of medical control of the condition, whether the person may be depended upon to continue medication if continued medication is prescribed and all other relevant, competent, and material evidence." Of the first 212 applications in Ohio (3), 108 licenses have been issued, 30 rejected largely because of seizures occurring within the prior twelve months, and 74 have not been processed completely. From the passage of the law in June, 1957 through February, 1958, only one hearing was necessary before the State Board of Review.

In terms of numbers alone, accidents involving epileptics are relatively rare. Alcoholics are responsible for hundreds of times as many automobile accidents as are epileptics; yet alcoholics are not required to make any record of their condition on their

license applications nor are they reported to state authorities merely because of alcoholism (13). Epileptic attacks have been causative factors in automobile accidents and train mishaps, yet one study by Hierons (10) indicates that diabetics receiving insulin and people with coronary artery disease are likely to have at least as many accidents. Few advocate the refusal of driving licenses to persons with coronary disease or diabetes. Hierons remarks that professional drivers who have had even one epileptic attack should not drive public transport vehicles. Another British study (29) of professional drivers called for the draft revealed that five of 1,276 commercial drivers had epilepsy or periods of unconsciousness.

Some cases will be presented to illustrate the difficulties which arise in connection with driving and epilepsy.

In Eleason v. Western Casualty and Surety Company (36), a truck driver became unconscious while driving and injured the plaintiff. He did not know that he had "epilepsy," but he did know that he was subject to fifteen-minute fainting spells. In the village where he lived, he had been known to have seizures for six years. The court ruled that driving a truck under these conditions, endangering the lives of others, was negligence *per se*. The fact that the driver did not know the technical name, epilepsy, was not controlling; the driver knew that he was likely to become incapacitated while driving. The employer was held free from negligence in hiring him, as he had made a reasonable investigation when hiring him and did not know of the driver's condition.

People v. Freeman (48) was a case in which the defendant, having a history of epilepsy, felt unwell while visiting, and on driving home zoomed through a stop sign at sixty miles an hour with a resultant accident. He pleaded that he was unconscious at the time of the accident. The court stated that if the defendant knew of his past epileptic condition and was in normal mental condition when he left, driving a car constituted criminal negligence; but if he was already in an epileptic state when he left the house, he was not responsible for his actions. Thus the mental condition of the driver was a question of fact for the jury.

In Commonwealth v. Irwin (35), an epileptic appealed a lower court decision suspending his driver's license. He had had no seizures in eight years. (Pennsylvania has required a seizure-free period of three to five years.) The court said, "There will, no doubt, be common agreement that a person afflicted with epilepsy is incompetent or unable to exercise reasonable or ordinary control over a vehicle on the public highway." The court indicated, however, that with a doctor's certificate that the epilepsy was under control, the petitioner would be reeligible for licensure. (In point of fact his request was turned down on a technicality as he had not made his appeal within the requisite thirty days.)

When a parent purchases a car for an adult epileptic son, the parent may be liable for a subsequent accident (37). The court held in this case that it was negligent to endanger the lives of others by placing a potentially dangerous instrumentality in the hands of an epileptic.

Contrary to the Eleason case, in Lucas v. City of Juneau (43) the municipal employer was held liable for an accident caused by an epileptic ambulance driver employed by the city. An accident resulted when he lost control of the vehicle during a seizure. In another case (58), it was a question for the jury whether a taxicab company had used due care in hiring its drivers when one of them caused an accident during an epileptic attack. The taxicab company was held not liable for the accident itself, that it was not an insurer of its passengers, and that there was no negligence on the part of the taxicab company immediately connected with the accident.

In another action (47), the defendant was prosecuted for criminal negligence for op-

erating an automobile with the knowledge that he was subject to epileptic seizures. During an attack, his car went out of control and killed four people. The defendant had had a long history of seizures (from 1950 to 1955), was on medication, and still had ten to twenty seizures a year. In this case a judgment of conviction was set aside on the procedural ground of doctor-patient privilege, that the doctor who had treated him at the emergency hospital could not testify as to diagnosis without the patient's permission.

EPILEPSY, WORK, AND WORKMEN'S COMPENSATION

The problem of the epileptic at work is minimized because of the simple fact that it is almost impossible for an epileptic to obtain or keep employment. Typically, after one seizure at work the employee is discharged. Practically speaking, it is true that epilepsy presents a menace in certain occupational situations, and for the protection of the epileptic and others, he should not work at heights, driving, or with moving machinery—unless the epilepsy is completely under medical control. Regardless of control, if a person mentions that he has had epilepsy, he usually cannot find work and so becomes a charge either on society or on his family. With the present enormous rehabilitative effort directed to other handicapped persons, the epileptic who is a basically able person can still find little help. The social and cultural stigma is such that all he may find is sympathy, despite the fact that there are innumerable desk jobs or jobs involving manual labor of the fine variety which he may be able to perform as well as anybody. A United States Department of Labor study of 11,000 impaired workers (including all kinds of physical impairments) and 18,000 matched unimpaired workers showed no significant difference between the two groups either as to performance or accident experience. In eight years (1945 to 1952), the average number of closed cases per year involving epileptics before The Workmen's Compensation Board of the State of New York was ten cases per year, while the total number of cases averaged 100,000 cases per year. Thus epilepsy is seen to play an insignificant role statistically. The safety director of the Ford Motor Company reported that, "accidents are so low among epileptics that it would not convey any data worth looking at. Out of 165 epileptics, over a period of many years, we do not have one accident case worth mentioning." Seizures did occur on the job, but none resulted in accidents. Another study of factory workers showed a slightly higher incidence of accidents in epileptic employees, but of no statistical significance.

Aside from the stigma of epilepsy and the fact that epilepsy makes certain jobs impractical (driving trucks, sales work, etc.), one great factor in maintaining unemployment is The Workmen's Compensation law. Employers do not wish to risk increased costs, yet there have been various methods of countering this—waiver clauses, "Secondary Injury Funds," and the like (2).

Two important problems facing compensation boards are: 1), cases where injury is caused by epilepsy and 2), cases where injury results in epilepsy. There is disagreement as to the compensability of an injury due to an epileptic attack at work. In Rockford Hotel Company v. Industrial Commission (52), it was stated, "A majority of courts, American and English, hold that, if the injury was due to the fall, the employer is liable, even though the fall was caused by a pre-existing condition." A minority view states that, to be compensable, the injuries must result from the employment and not be caused by the seizure (31).

Another example of the majority view is the Stasel case (54) (in accord: United States Casualty Co. v. Richardson (56)) where an employee, unaware that he had grand mal epilepsy, was injured in a seizure and the Compensation Board declared that the injury was compensable.

A case where damages were denied because of the failure of the injured party to submit to examination was that of Cole v. Town of Miami (34). Here an award of the Industrial Commission denying compensation on the ground that the defendant had failed to show that his Jacksonian epilepsy was a result of his injury, was not arbitrary and unreasonable, where there was testimony that his condition might also have been the result of a brain tumor or neurosyphilis, and where claimant had refused to submit to a medical examination to determine the cause of his condition. The court felt that the commission was not unreasonable in asking that the claimant submit to an encephalogram and a spinal Wasserman to clarify the cause of his Jacksonian epilepsy (he had had three seizures). In cases of this type the essential testimony is usually that of the physician. Oftentimes the physician cannot definitely answer "yes" or "no," but must evaluate the case according to probability and experience. In Kennedy v. Holmes Construction Co. (40), the neurosurgeon stated that the most likely cause of the convulsions was a blow on the head, while the plaintiff's personal physician definitely affirmed that such was the case. The court said that "medical testimony is essential here; a causal connection between the original injury and the claimant's present disability must be shown by positive expert opinion and evidence of such quality and quantity as to amount to more than a probability, conjecture, or guess.... The medical expert must testify, in effect, that in his professional opinion, the result in question came from the cause alleged." The court held that while the opinion of the expert alone might not be sufficient to support an award, the opinion of both physicians was ample.

In Puskarewicz v. Prudential Insurance Co. (51), the case involved a "double indemnity for accident" clause in an insurance policy. The deceased had an epileptic seizure in the bathtub and was found dead. One medical witness felt that the epilepsy had caused a lung hemorrhage which was responsible for the death, while the opposing medical witness felt that it was more likely that he had drowned. The policy stated that for double indemnity, the death must be solely through accidental means and "independently of all other causes." Here it was obvious that the insured's death would not have occurred except for the epileptic seizure, and the court held that there was insufficient testimony to indicate death by accidental means. In Hochberg v. Travelers Ins. Co. (39) it was left to the jury to decide under a disability policy whether frequent seizures resulted in total disability, and lay opinion of past behavior was relevant as well as medical opinion. An interesting case was McPartland v. State (45), where a hospitalized epileptic, having a seizure, fell into a shallow tub used for watering cattle and drowned. At the time, the deceased had improved and was under consideration for discharge from the state hospital. No recovery was granted since this was not an event which would have reasonably been anticipated by prudent management. In a case involving prenatal injury (42), damages were refused to a child whose seizures were alleged to have been the result of prenatal injuries before the fetus could have been born viable.

EPILEPSY IN CRIMINAL AND CIVIL CASES

Various aspects of the relationship of epilepsy to legal problems have been touched upon in the two outstanding books on psychiatry and the law (4, 9). The most fascinating aspects, of course, deal with the mental responsibility of the epileptic.

Davidson (4) states, "In itself, epilepsy cannot exculpate an offender. If a crime is committed during a psychomotor equivalent period or during an epileptic fugue, the patient's accountability is measured by determining whether he acted as if he knew he had done something wrong.... Crimes committed in a postconvulsive confusional state are evaluated in terms of the patient's ability

to appraise the wrongfulness of the act. If the confusion is great, it will be obvious that the offender did not know what he was doing. Accidental injuries inflicted to property or bystanders are not criminally charged to the epileptic if the injury occurred during the acute phase of a convulsion, since obviously there was no intent to do wrong." As far as competency is concerned, epilepsy usually does not affect one's mental capacity to carry on his affairs. Exceptions occur when one is, in addition, mentally deficient or insane, and also when the epileptic transacts business in the midst of a confusional or psychomotor episode. Such occasions are difficult to evaluate. Evaluation would depend on a thorough study of the circumstances, a history of how prolonged and frequent the episodes have been previously, and the person's characteristic behavior during these episodes.

In any case, the behavior during the crime, past history, and electroencephalographic records would be the subject of study. Friedman (8) states that, for the epileptic to be not mentally responsible, it must be shown that the person had epilepsy, that he was under the influence of the seizure during the act, and that the act must be a result of the seizure.

Thus, "epilepsy may be considered, while it lasts, as a state of insanity during which the patient is deprived of reason and judgment, and at the same time of sense and consciousness, and is wholly incapable of doing anything" (41). An epileptic is excused from criminal responsibility for an act done in violation of the law only when he is unconscious of the act which is committed (49). (In this case, it was stated that in disorderly conduct, when it is undisputed that the individual had an attack of epilepsy and does not remember committing the act charged against him, conviction is unauthorized; with a reversal of conviction in a lower court.)

It must be kept in mind that epilepsy *per se* is not insanity and, in People v. Syjut (50), it was stated that epilepsy did not establish that a defendant who was subject to epileptic seizure was insane or feebleminded so as to warrant an adjudication of insanity, and that the defendant was unable to stand trial for murder. An extremely interesting case discussed in full detail (28), was that of an infanticide related to psychomotor autism. Here the woman involved refused to plead "not guilty by reason of insanity," pleaded guilty and was given five years' probation. Another case of epilepsy in association with murder is reported in Guttmacher's book (9, p. 164). In another case (55) a defendant during a murder trial had repeated seizures during the trial with recesses held after the attacks so that he might recover. On appeal for conviction for first degree murder, the court said that the evidence was not held to support the contention that the defendant epileptic was not mentally present at all times during the trial.

The question of civil responsibility for acts performed during seizures is answered by contrasting opinions. In Sauers v. Sack (53), the defendant, in reviving from a seizure, committed an assault and battery for which the plaintiff sought damages. The court stated, "In a suit for damages occasioned by an assault, the fact that the defendant was in the throes of an epileptic fit and did not have intent is no defense to the action. . . . In a tort by an incompetent upon a non-consenting plaintiff, intent is no ingredient of the tort; insanity or incompetency is no defense to a civil action for mere compensatory damages." This seems to be a minority view. Several American decisions *contra* have been reported (20) and in a note discussing the above case (21), it was stated that, "no action in tort has been successfully maintained against a child or insane person, incapable alike of negligence and culpable intent, in England since 1456."

CONCLUSION

This paper has discussed various medicolegal aspects of epilepsy and has been con-

cerned with the relation of medical factors to legal principles. Statutes and common law relating to epilepsy have been surveyed with the presentation of illustrative legal cases.

It is evident that the advance of medical knowledge has brought about a need for legal reform in the handling of the epileptic. Most laws pertaining to the marriage of the epileptic are now clearly obsolete. Both for scientific and practical reasons, it is the opinion of the writer that all restrictive legislation should be removed from the statute books. The rule that marriages may be terminated by divorce or annulment where one party has fraudulently concealed epilepsy from the other is a reasonable one. Laws regarding sterilization should also be repealed, although at present they have little practical effect. The primary reason for new legislation in this regard would be to lessen the social stigma which is now sanctioned by statute.

Already basic law involving driving is changing in many states with the result that the privilege of driving is increasingly evaluated on individual grounds based on clinical condition, rather than on sweeping exclusions without scientific rationale.

Problems involving workmen's compensation and legal responsibility seem at present to require no great change in the law. The essential element in these spheres is the utilization of thorough medical evaluation in the disposition of cases involving epileptics.

The many ramifications of the problem of epilepsy and the law as discussed here illuminate the need for physicians to take a more active role in helping the courts of law to keep abreast of advances in medical knowledge; for the highest standard of justice is that based on reason and knowledge.

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BRIEF COMMUNICATION

PARASAGITTAL SYNDROME PRODUCED BY TRAUMA

DEWEY K. ZIEGLER, M.D.[1]

Knowledge of the function of the parasagittal area of the cerebral hemispheres has been obtained by the use of several techniques: study of primates with extirpative lesions of the area (1), stimulation of the area in the exposed cortex in the epileptic patient (4), use of the evoked potential technique (5), study of patients with neoplastic or traumatic lesions of the area (2, 3). The clinical diagnosis of a parasagittal lesion still remains a difficult one. The following case is reported because trauma to the skull created a lesion apparently well localized to the mid-portion of the parasagittal area in the region of the sensorimotor strip, and produced a syndrome rarely seen with brain lesions.

The patient, a man of fifty years and in good health, became depressed while under the influence of alcohol and shot himself in the head with a pistol. From the appearance of the X-rays, he must have held the pistol barrel almost tangential to the skull at a point about mid-way on a line between the ears. Probably due to a defective charge, the bullet traveled only far enough to lodge itself in the tables of the skull, producing a slight depressed fracture (Figures 1 & 2). The latter was elevated in an operation performed by Dr. Robert Forsythe. The dura was not found to be torn, and was not entered. The post-operative bone defect is shown in Figure 3.

The patient denied any weakness or sensory disturbances prior to the injury, but post-operatively he noted extreme weakness and unsteadiness of gait. There was no disturbance of function of the arms, of micturition or defecation, nor had the patient had any paresthesias in arms or legs. Five days post-operatively the patient showed the following findings:

1. Markedly wide-based gait with staggering to either side.

2. Marked flaccid weakness of distal muscle groups in both legs, right more than left, with a lesser degree of weakness in the thigh flexors and extensors. There was almost complete flaccid paralysis of the dorsiflexors and plantar flexors, evertors, and inverters of the right foot. The gait was ataxic, hesitant, and displayed foot drop; it had been thought hysterical.

3. Sensory examination revealed no marked loss of diminution of pain or touch sensation in the legs, but a variable and inconstant response to pin-prick and touch stimulus on both legs to the mid-thigh level. There was failure to appreciate multiple, repeated pin or touch stimuli as such. Vibratory sense was intact. Position sense was extremely impaired in the toes of both feet, with some impairment in ankles and knees. Two point discrimination was severely impaired to the thigh level bilaterally. There were no sensory abnormalities in the arms, head, or trunk.

4. Tendon reflexes were normally brisk and equal in the arms; in the legs the right patellar and Achilles reflex were more active than the left, but all were within normal limits. There were bilateral Babinski signs.

5. Cranial nerve function was not impaired. Coordination was normal in the upper extremities, and was not impaired in the lower extremities when tested with the eyes open.

6. Mental status: The patient appeared to be of average intelligence, and was alert, cooperative, and desperately anxious and eager to regain full function of the legs and to be reassured that the function would

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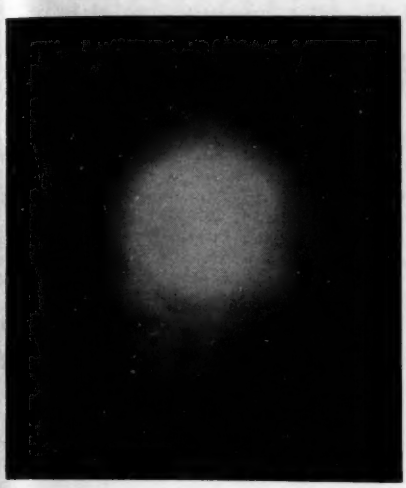

FIG. 1. X-ray demonstrating depressed skull fracture discussed in text.

FIG. 2. X-ray demonstrating depressed skull fracture discussed in text.

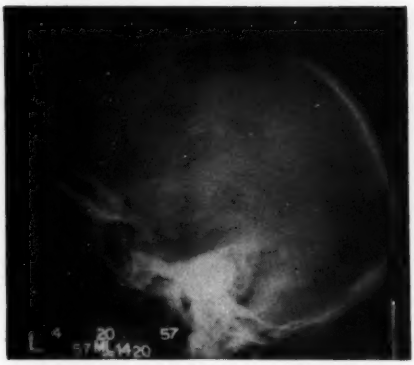

FIG. 3. X-ray demonstrating post-operative bone defect.

return. There was no indication of severe depression, no history of psychotic or grossly neurotic behavior in the past.

The patient showed no change during four weeks after the injury, save for two generalized seizures. He then moved away and became inaccessible for further observation.

COMMENT

Although the syndrome produced by damage to the parasagittal region is known, most of the lesions producing this syndrome in man, have been tumors—particularly meningiomata. In the present case, with the bullet failing to penetrate the skull completely, and not tearing the dura, the presumption is that the area of brain damage would be quite localized to the area immediately underlying the bullet hole—the mid portion of the parasagittal area. The findings; bilateral mild, flaccid weakness with brisk reflexes and Babinski signs, loss of position and other "cortical" sensation in the legs, ataxic gait, are those to be expected with damage to the areas nearest the midline of sensory-motor strips of both hemispheres. The inconstant loss of pin-prick and touch sensation in the peripheral part of the legs is an unfamiliar manifestation of brain injury, but has also been reported by Marshall (2). The flaccid weakness with bilateral foot drop and sensory loss in the legs constitute one of the rare situations where a brain syndrome simulates a peripheral neuropathy.

It is rare for neoplasia to produce symmetrical restricted lesions of this type, and the present case provides an interesting natural experiment to demonstrate the effect in man of trauma restricted to the midparasagittal area. That this brain syndrome is a comparatively unfamiliar one, is at-

tested to by the fact that the patient's gait had been thought, on preliminary examination, to be hysterical.

SUMMARY

A case is reported in which a bullet lodged in the tables of the skull near the midline produced bilateral flaccid weakness, and "cortical" sensory loss in the legs, presumably due to localized damage to the mid portion of the parasagittal lobule bilaterally. The possible confusion of this rare brain syndrome with hysteria or peripheral neuropathy is emphasized.

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